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FUTURE FERTILITY OF AMERICAN WOMEN
P. K. Whelpton

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MARRIAGE AND FAMILY RESEARCH
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SYMPOSIUM ON GENETICS AND INTELLIGENCE
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Vol. 1, No. 1, March 1954

Eugenics Quarterly

EDITORIAL COMMENT

Eugenics Quarterly Succeeds Eugenical News

With this issue EUGENICS QUARTERLY becomes the new name of the official journal of the American Eugenics Society, replacing EUGENICAL NEWS. Vol. 1, No. 1, March 1954 commences another phase in the history of the publication. Thirty-eight volumes of EUGENICAL NEWS have been published since the journal was started. The following excerpt from a history of the American Eugenics Society (published in the December 1946 issue of EUGENICAL NEWS) traces briefly the story of the journal:

"In 1920 the Eugenics Research Association planned a *Journal of Eugenics*, about 400 pages a year. In August of that year the EUGENICAL NEWS was acquired from the Eugenics Record Office with the idea of using it as a temporary bulletin until the *Journal of Eugenics* was ready for publication. But the *Journal* was never published and the EUGENICAL NEWS was the official organ of the Eugenics Research Association until that organization was discontinued in 1938.

The December 1945 number of EUGENICAL NEWS completed Volume XXX, thirty years. No. 1 of Volume 1, published in January 1916 by the Eugenics Record Office at Cold Spring Harbor and edited by Dr. Charles B. Davenport and Dr. H. H. Laughlin, was announced as a bi-monthly periodical which would be a medium of intercommunication between eugenists and a general eugenics newspaper. Its subscription price was 25 cents for 6 numbers. The first number had 4 pages. The reception of the new magazine by interested readers encouraged Dr. Davenport and Dr. Laughlin to change to a monthly, and 12 numbers with a total of 94 pages were issued in 1916. It was changed to a quarterly when the American Eugenics Society became owner in 1939."

The Role of the American Eugenics Society

The change in name of the Society's journal comes at a time of rapid advance in the science of human genetics, and of a new appreciation by the medical profession of the importance of hereditary factors in disease and defect. The brief period since the war has seen the successful start in this country of the *American Journal of Human Genetics*, the publication of a number of books here and abroad on the subject of medical genetics, and numerous well-attended scientific conferences in this field. Comments in the press and in various journals indicate a growing public appreciation of the significance of this subject. At a genetic conference in Italy, the Pope made a statement on the importance of studies in human heredity and the high moral aim of eugenics, though he disagreed with such means as sterilization and birth control.

In this change in the scientific, medical and even lay climate of opinion, the Eugenics Society has played a minor part. It is perhaps as well that this is so. The base for negative eugenics is being laid by a group of medical geneticists here and in other countries. Public health will require that it be followed up by practical applications very soon after the scientific justification has been demonstrated. The Eugenics Society may help by encouraging research and the establishment of heredity clinics. But the attitude of the doctor will be the determining factor, and eugenic propaganda can be useful chiefly in promoting public support of measures generally accepted by leaders of the medical profession. As medical knowledge develops, there will be an increased protection against hereditary disabilities; diagnosis will be earlier;

the number of children in afflicted families will be smaller. These steps will tend to bring about a reduction in many types of defect. Eventually, as it becomes possible to detect an increasing number of carriers of recessive genes, it may be possible vastly to diminish the heavy burden of the socially inadequate and other defective hereditary types. The continuous fight against the spread of new injurious mutations will go on as it always has in the past, but the weapon will be the merciful one of selective control of births, instead of nature's harsher method of selective deaths. And birth selection, properly applied, can be far more effective.

For the first time negative eugenics seems to be assured of moving toward its goal with respect to the most severe diseases mainly due to heredity. But even if all injurious mutations were destroyed immediately as they appeared, there would be no change at the upper levels of human activity. The great possibilities for improvement lie in changing the distribution of births among normal people, so as to increase the proportion of children at the higher levels of normal capacity, while reducing the proportion of those at the lower levels of normality. This would raise the average level of the whole and greatly increase the possibilities for a better human society and for individual and social happiness. This is the great field for eugenic advance, and here lies the opportunity of the Eugenics Society.

Long before science is able to tell us how heredity operates at the higher levels of capacity, great improvements can be made in the distribution of births. Limited but effective programs can be developed within the minimum of scientific knowledge now available and enlarged as knowledge increases. The difficulties lie with the education of the public. The public has first to learn that race and social classes are relatively of little importance as indices of hereditary capacity. This is not an easy lesson to learn. We all like to think that our own group of people has superior qualities. The early eugenists were not alone in the admiration of their own race and class. Such ideas have been held by many societies and classes since earliest times and are quite general throughout the world today. In the United States and the democracies of western Europe their hold is weakening. It should be the most immediate task of eugenics to substitute for this ancient form of self-admiration the knowledge that hereditary capacities are widely scattered throughout the human race, among families within every social class and within every geographic area. This is the message of science. It is implicit in the ideals of Christianity and of western civilization. The Eugenics Society can properly emphasize this message with a new purpose in mind, the improvement of hereditary capacity from one generation to another. The largest families should be found, not as a characteristic of particular races or social or economic groups, but among couples in every situation who give evidence of socially valuable qualities in response to the particular circumstances of their own lives. Natural selection, by either births or deaths, works in general categories, on averages, and it seems inconceivable that a large-scale, voluntary selection such as that proposed would not on the average tend toward genetic improvement from one generation to another. At the same time, on the assumption that thoughtful, intelligent persons who eagerly accept their responsibilities as parents do the best job with their children, an increase in the proportion of births to parents with these qualities would also raise the level of home influences and thus improve acquired qualities of our nation's children.

There are indications in recent scientific studies that such a pattern of births can and does emerge under the proper conditions of the economic and psychological environment. But it is a slow and uneven business. It should be the major role of the Eugenics Society to accelerate this change.

It is a challenging task.

There can be no arbitrary decisions as to who should and who should not have children. The parents alone must make the choice. An arbitrary eugenics would at best have injurious effects which might outweigh its genetic advantages. It would also be a dangerous undertaking. In a despotic state eugenics could only be an arbitrary breeding of certain human types. Not only is this repugnant to the traditions of our civilization; it is exceedingly dangerous because no man is qualified to tell who should or who should not survive. This is why eugenics can only succeed in a democracy. In fact, an improved race of human beings would be one with greater variety at a high general level. The process of selection must spring out of the conditions of society. It must come as the spontaneous response of individual couples to the pressures and the aspirations of the environment in which they are living. It is the task of eugenics to help create such an environment. This is a reasonable and a possible task. Some recent changes have tended in this direction, and the social atmosphere is favorable to this ideal. But there are many very real difficulties.

Only a few of the factors which make for differences in size of family are known at present. For example, we know that when contraception is generally practiced class differentials in births tend to disappear or be reversed; we know that late marriage makes for smaller families; we can presume that income tax exemptions for dependents have some eugenic advantage. But we know little about the psychological considerations or the early influences which encourage young parents to have another child. Fortunately these are things which can be studied. And as they are better understood, influences can be developed for an increasingly sound selection of births.

Educating public opinion along these new lines of thinking is going to take a long time. This educational work must start with the leaders in education, in the biological and social sciences, in the medical and social sciences, and in related fields. As the possibilities of a scientific and democratic eugenic program become more widely recognized, it will receive the support of thoughtful people of all races and of all religions.

In seeking such a goal, eugenics sets itself a high and an achievable aim. Every economic and social change, every change in the psychological environment, which tends to a better distribution of births, will by so much improve the environment and the average genetic capacity of the next generation. Even a very little change may be important in facilitating further changes in the next generation. Every increase in scientific knowledge of the factors which affect decisions as to size of family will lead to new interest and a better understanding of the problem.

It should be the major function of the Eugenics Society to help discover and carry through those changes in our society which will tend toward a better distribution of births. That our children should be born in homes where they will get the best of home environments, and the best chance for a eugenic heredity, should be a lifetime challenge to all those interested in our country's future.

FUTURE FERTILITY OF AMERICAN WOMEN

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FROM 1946 TO 1952 the annual number of births in the United States varied between 3,400,000 and 3,900,000, compared with previous records of 3,100,000 in 1943 and 3,050,000 in 1921. A new high mark of nearly 4,000,000 has just been set in 1953, which is nearly 75 per cent above the figure for 1933 — twenty years earlier. During the last eight years the crude birth rate has been between 24 and 27 per 1,000, compared with between 18.4 and 22.7 during 1928 to 1945. No such great upsurge in fertility has occurred previously in the United States.

This postwar "baby boom" soon led many people to think not only that there had been a rise in the current fertility of American women, but also that an increase in the average size of their completed families had begun.[1] By now it is commonly believed that the longtime downward trend in average size of family, which started at least as far back as the early 1800's, was reversed a few years ago. In consequence, the number of births in future years is expected to be larger than the number in recent years. On the other hand, at least one demographer expressed the opinion as late as a year ago that in spite of the recent "baby boom" the average number of births per completed family had not stopped decreasing, and that the shifts which had occurred in the timing of births would inevitably bring within five or ten years a reduction in numbers of births amounting to ten per cent or more.

Usually when there are such seemingly contradictory opinions about the direction of change, one or the other is wrong, or else both are wrong because no change is taking place. Is this true here, or is it possible that the average size of families is increasing and decreasing at the same time? This question will be discussed in Sections I and II. Finally, in Section III the outlook for changes in annual fertility during future years will be considered briefly.

During recent years the measurement of fertility trends in the United States has been assisted materially by the preparation of cohort fertility tables which trace the birth performance through the childbearing period of the women born in different years. These tables provide most of the statistical basis for this article.[2] They are available as yet for native white women only, which means that most of the following discussion will relate specifically to these women. Nevertheless, the conclusions should be fairly applicable to all white

[1] "Average size of completed families" is used here (somewhat loosely) as equivalent to "average number of live births per woman living to the end of the childbearing period."

[2] These tables in summary and detailed form will be contained in the writer's monograph, *Cohort Fertility: Native White Woman in the United States*, which is scheduled for publication in February, 1954, by the Princeton University Press. The summary tables in this article are based on the detailed tables in the monograph. They differ from the summary tables in the monograph because data for 1949 and 1950 could be used in their preparation, which permitted reference to five-year time periods consisting of 1945–49, 1940–44, etc. instead of 1944–48, 1939–43, etc., and to the situation on January 1, 1951. The same grouping of cohorts (1875–79, 1880–84, etc.) is used in all cases. This means that cumulative birth rates of cohorts are shown up to exact ages 15–19, 20–24, etc. in the text and summary tables of the monograph, but up to exact ages 16–20, 21–25, etc., or 17–21, 22–26, etc., in this paper. In both cases years and cohorts are grouped by fives in order to condense the discussion.

women and also to all women in the United States, because native white women have constituted since 1920 at least 84 per cent of the former and 74 per cent of the latter at ages 15–44. The tables in question show current and cumulative fertility rates for each age from 15 to the end of the childbearing period (or the age reached on January 1, 1951, if less than 47) for native white women in the cohorts of each year from 1900 to 1935. Deficiencies in the registration system make it necessary (a) to begin the tables for the cohorts of 1875 to 1899 with cumulative rates for the age reached at the beginning of 1920, and to show current and cumulative rates by single years of age only from 1920 to the end of the childbearing period, and (b) to omit entirely the cohorts of 1874 and earlier years. The discussion here is influenced accordingly.

I. Has the Downward Trend in Family Size Been Reversed?

That there has been a large decrease during the last 30 years in the average number of births per native white woman living to the end of the childbearing period is shown clearly by the cohort fertility tables. Among the women in the cohorts of 1875–79 who lived to age 47,[3] there were 3,497 births per 1,000 women. (See Table 1.) In contrast, the final cumulative birth rate for the cohorts of 1900–04 is 2,483, a drop of almost 30 per cent. Moreover, it is fairly certain that the decline was not interrupted among the cohorts of 1875 to 1904.

The women in the cohorts of 1905–09 were 42–46 years old at the end of 1950 (the latest year for which tabulations are available for births by order of birth to women in different cohorts), hence, there is very little likelihood of their bearing many more children during 1951 or later years. Judging from their cumulative birth rate up to January 1, 1951, it is almost certain that their final rate will be between 2,325 and 2,350, which is five to seven per cent smaller than that of the cohorts of 1900–04. Within the 1905–09 group there has also been a gradual decrease in fertility. This means that the past downward trend in the average number of births in completed families almost certainly will continue to the cohort of 1909, whose women averaged 45 years of age on January 1, 1954.

That the downward trend will be reversed among later cohorts can be predicted with a high degree of assurance. It is now known that in the cohorts of 1910–14 there were 2,200 births per 1,000 women by ages 37–41 (*i.e.*, by January 1, 1951) compared with 2,188 for the cohorts of 1905–09 by these ages (*i.e.*, by January 1, 1946). Moreover, each cohort from 1910 to 1914 had a larger cumulative rate by each age from 34 to that reached on January 1, 1951 (*i.e.*, 37 to 41) than did the 1909 cohort, with a tendency for the differences to be larger for the later cohorts. Unless the women in the cohorts of 1910 to 1914 bear at least 15 per cent fewer children after ages 37 to 41 than those in the 1909 cohort, which could happen but now appears quite unlikely, the 1909 group will have the dubious distinction of having been the least fertile, and the 1910 group can claim to have started an upward trend. On this basis, the year by which the longtime decrease in the average size of completed families will have been reversed can be determined by adding to 1909 the age at which women reach the end of the childbearing period. If this age is taken as 47, the trend will have been reversed by 1956; if 45 is used, the turn will occur in this year — in 1954.

A glance at the cumulative birth rates by order of birth, and at the distribution by parity (number of children ever borne) of the women in different cohorts who live to

[3] In computing the basic tables it was assumed that no births occurred after a woman's 47th birthday. The very small number of births at later ages according to the registration records were arbitrarily classified as occurring during age 46.

the end of the childbearing period, confirms the truth of the general belief that the decline in average family size has come about in important degree through a decrease in the proportion of large families. As shown in Table 1, the final cumulative birth rate was 3,497 for the 1875-79 cohorts and 2,483 for the 1900–04 group, a decrease of 1.014. Slightly over one-fifth of this occurred among eighth and higher order births (from 361 to 133) and nearly as much for sixth and seventh births (from 403 to 183). The largest reduction for a single birth order is that of 157 for fourth births, closely followed by 143 and 142 for third and fifth births, respectively. For first births the decline was only 34. The *relative* size of the reductions rises sharply from four per cent for first births to 63.2 per cent for eighth and higher order births. (See Fig. 1.)

TABLE 1

FINAL CUMULATIVE BIRTH RATES, BY ORDER OF BIRTH, FOR NATIVE WHITE WOMEN IN THE COHORTS OF 1875–79 TO 1905–09[1]

Cohorts of	*On January First of*	*Total Births*	*Order of Birth*						
			1st	2nd	3rd	4th	5th	6th & 7th	8th & Higher
			Cumulative Birth Rates						
1905–09	1952–56	2,325–2,349	775–779	586–590	359–363	219–223	137–141	146–148	103–105
1900–04	1947–51	2,483	773	592	389	250	163	183	133
1895–99	1942–46	2,740	785	624	432	294	198	233	174
1890–94	1937–41	2,938	791	641	462	325	226	276	217
1885–89	1932–36	3,121	796	655	485	352	252	318	263
1880–84	1927–31	3,306	802	669	509	379	279	360	308
1875–79	1922–26	3,497	807	682	532	407	305	403	361
			Decrease from Cohorts of 1875–79 to 1900–04						
Numerical		1,014	34	90	143	157	142	220	228
Per cent		29.0	4.2	13.2	26.9	38.6	46.6	54.6	63.2

[1]From Tables A and G of *Cohort Fertility: Native White Women in the United States.* The figures for the cohorts of 1905–09 are based on the actual numbers of births to the women in these cohorts up to ages 42–46 and estimates of the numbers during the remainder of the childbearing period.

The final distribution by parity of the women in a cohort can be obtained from the differences between the final first birth rate and unity, and between final rates for successive pairs of first and higher order births. The proportion of childless women rose slightly, from about 19.3 per cent in the cohorts of 1875–79 to 22.7 per cent in the cohorts of 1900–04. (See Table 2.) The largest increase occurred in one-child families (from 12.5 to 18.1 per cent), with two-child families in second place (from 15.0 to 20.3 per cent). Three- and four-child families were relatively stable, the former rising slightly from 12.5 to 13.9 per cent of this total, and the latter declining from 10.2 to 8.7. Successively larger decreases occurred for women bearing larger numbers of children. Those with five or six were only two-thirds as numerous (relatively) among the 1900–04 cohorts as among those of 1875–79, and those who had borne seven or more were less than half as numerous. Large families will be still less common among the 1905–09 than among the 1900–04 cohorts and two-child families more common. Little change will occur in the proportion with no child, or with one or three. (See Fig. 2.)

II. How Much Will the Average Size of Completed Families Change in the Future?

As shown in Section I, it is practically certain that average family size will be larger for the cohorts of 1910–14 than 1905–09, and still larger for the 1915–19 group. But will an upward trend continue

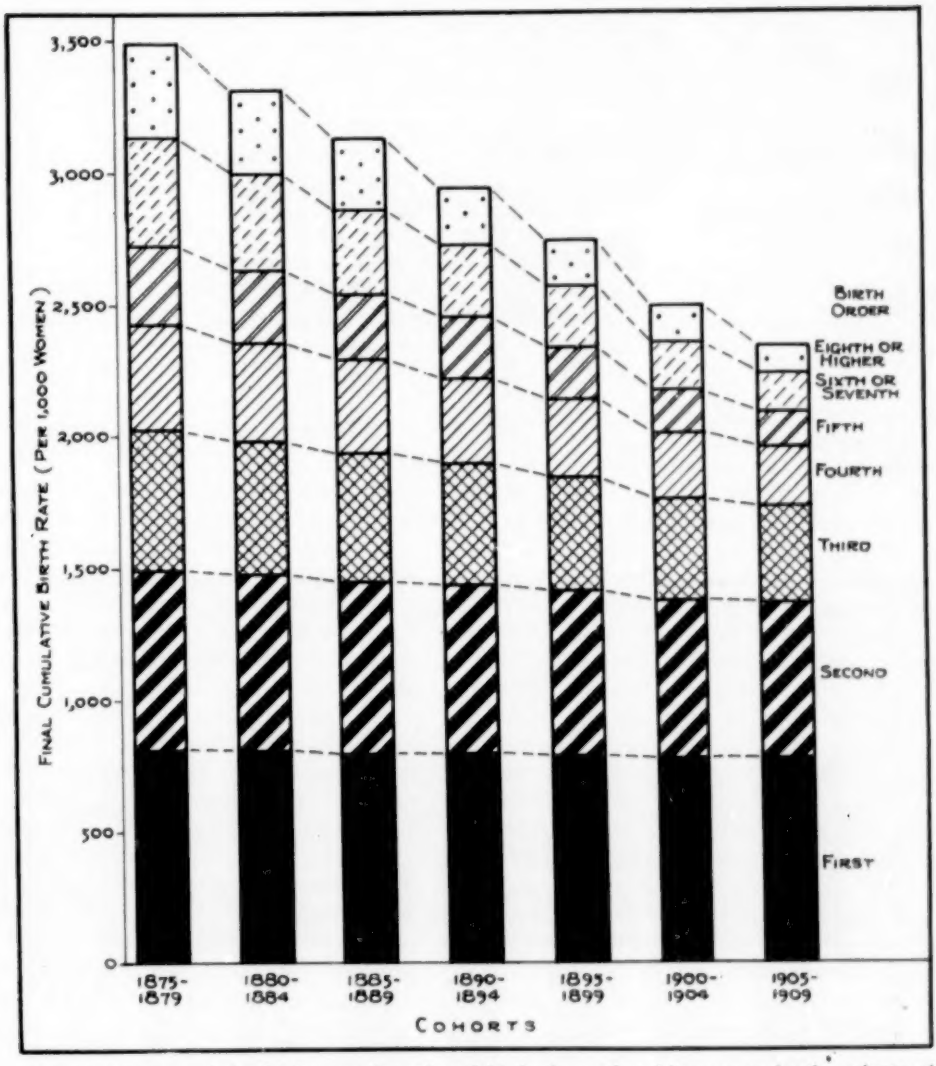

Fig. 1. Final cumulative birth rates, by order of birth, for native-white women in the cohorts of 1875-79 to 1905-09. (See Table 1.)

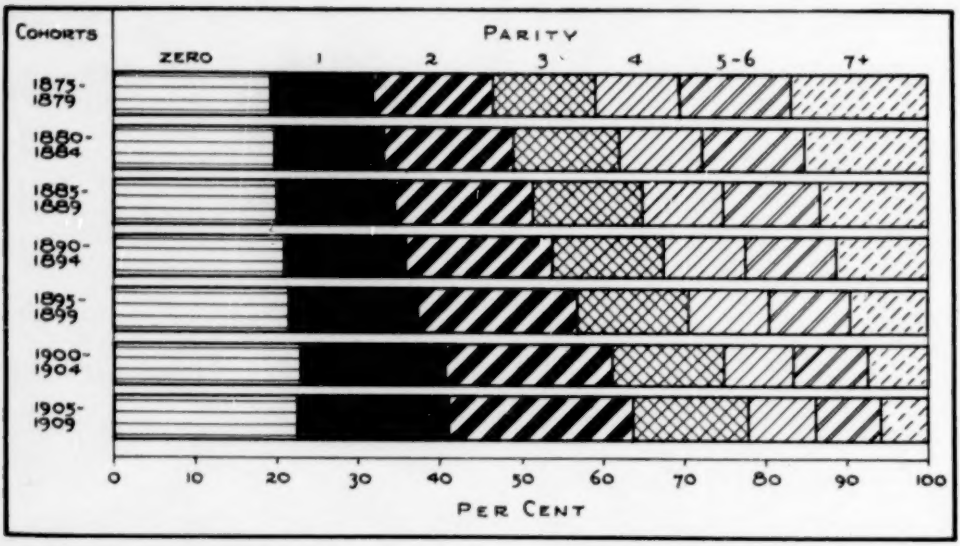

Fig. 2. Final distribution, by parity, of native-white women in the cohorts of 1875-79 to 1905-09. (See Table 2.)

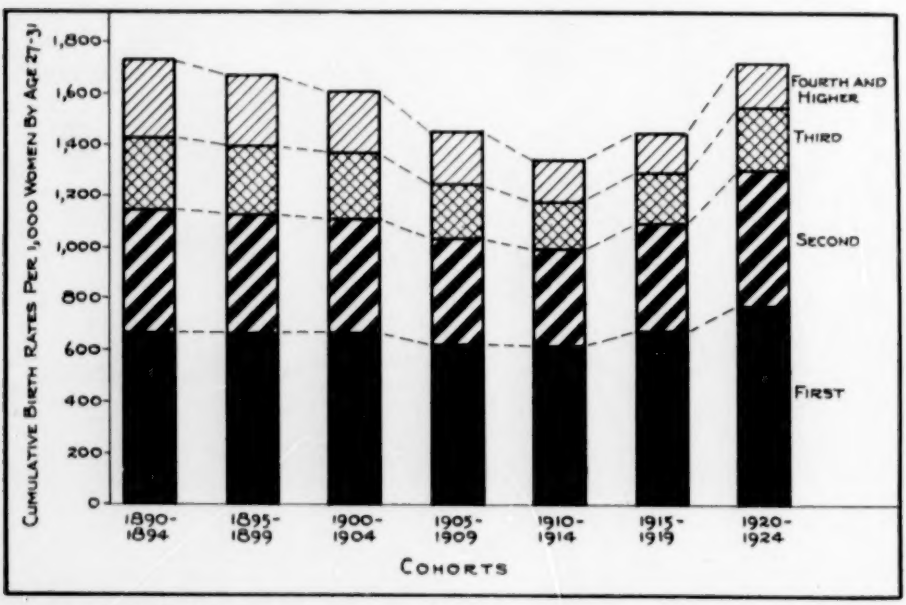

Fig. 3. Cumulative birth rates by ages 27-31, by order of birth, for native-white women in selected cohorts. (See lower section of Table 3.)

TABLE 2

FINAL DISTRIBUTION, BY PARITY, OF NATIVE WHITE WOMEN IN THE COHORTS OF 1875–79 TO 1905–09[1]

Cohorts of	On January First of	Parity						
		Zero	One	Two	Three	Four	Five & Six	Seven & Higher
1905–09	1952–56	22.5–22.1	18.9–18.9	22.7–22.7	14.0–14.0	8.2–8.2	7.9–8.2	5.8–5.9
1900–04	1947–51	22.7	18.1	20.3	13.9	8.7	9.0	7.3
1895–99	1942–46	21.5	16.1	19.2	13.8	9.6	10.3	9.5
1890–94	1937–41	20.9	15.0	17.9	13.7	9.9	11.3	11.3
1885–89	1932–36	20.4	14.1	17.0	13.3	10.0	12.0	13.2
1880–84	1927–31	19.8	13.3	16.0	13.0	10.0	12.8	15.1
1875–79	1922–26	19.3	12.5	15.0	12.5	10.2	13.5	17.0

[1]Computed from Table 1, and Tables A and G of *Cohort Fertility: Native White Women in the United States*. Parity as used here means number of children born alive.

among subsequent cohorts or will the former downward trend be resumed? And in any case, how large are the changes in family size likely to be? In trying to answer these questions, let us first examine some of the relevant facts which are available from the cohort tables.

It is known that the cumulative birth rate of the 1930–34 cohorts by ages 17–21, the 1925–29 cohorts by ages 22–26, the 1920–24 cohorts by ages 27–31, and the 1915–19 cohorts by ages 32–36,[4] are substantially larger than the rates of the 1910–14 group by the same ages. It is also known that the trend from cohort to cohort has been definitely upward during the last 15 years. These facts may be interpreted as pointing to an increase in average size of completed families at least from the cohorts of 1905–09 to 1930–34, or from about 1956 to 1981. Before accepting this interpretation or trying to estimate the extent of the increase, it is desirable to consider the cumulative rates for different birth orders.

The first comparison is that between the cohorts of 1910–14 and 1915–19, based on the rates up to ages 32–36. Those of the later group were larger by 8.0 per cent for first births, 14.6 per cent for second, 15.0 per cent for third, and 6.7 per cent for fourth, but were smaller by 2.4 per cent for fifth, 9.9 per cent for sixth and seventh, and 12.5 per cent for eighth and higher orders. (See Table 3, and Table 4 line *a*.) If these ratios were to apply at the end of the childbearing period, the average size of completed family would be 8.3 per cent larger for the later than the earlier group (see line *e*), and not 9.4. per cent as indicated by comparing the rates for all births by ages 32–36 (see line *a*). It is practically certain, however, that the ratios for first and second births will be appreciably smaller by age 47 than they were by ages 32–36. The latter would give the 1915–19 cohorts final rates of 872 for first births and 739 for second, which are far above those for cohorts as far back as 1875-79. (Compare line *c* of Table 4 and the line for the 1875–79 cohorts in Table 1.) On the other hand, it is possible that the ratios for sixth and later births will be somewhat larger by age 47 than they were by ages 32–36, although a decrease seems more likely in view of past trends for rates of these birth orders. Ratios for third, fourth, and fifth are likely to remain about as they were. Opinions will differ, of course, as to the size of all such changes, the direction of some, and the net influence on the average size of completed families. The writer thinks that the 1915–19 cohorts are almost certain to have more children than those of 1910–14, but that the increase will not exceed seven per cent.

An attempt to compare the final fertility

[4]These ages were reached by these cohorts at the end of 1950, the latest year for which the numbers of births are available as yet by age of mother by order of birth of child.

TABLE 3

CUMULATIVE BIRTH RATES TO SPECIFIED AGES, BY ORDER OF BIRTH, FOR NATIVE WHITE WOMEN IN SELECTED COHORTS[1]

Cohorts of	On January First of	Total Births	Order of Birth						
			1st	2nd	3rd	4th	5th	6th & 7th	8th & Higher
			Rates by Ages 32–36						
1915–19	1951	2,040	798	596	322	159	80	64	21
1910–14	1946	1,865	739	520	280	149	82	71	24
1905–09	1941	1,891	723	505	289	165	95	85	29
1900–04	1936	2,079	738	538	334	202	120	110	37
1895–99	1931	2,220	743	558	364	230	143	136	46
1890–94	1926	2,327	746	570	385	252	161	157	56
1885–89	1921	2,446	752	584	406	275	181	180	68
			Rates by Ages 27–31						
1920–24	1951	1,721	773	532	242	99	43	27	5
1915–19	1946	1,453	675	425	198	88	40	25	4
1910–14	1941	1,341	620	377	183	88	42	26	5
1905–09	1936	1,449	634	404	213	108	52	33	5
1900–04	1931	1,615	669	448	252	132	65	42	7
1895–99	1926	1,678	670	460	270	147	74	49	8
1890–94	1921	1,734	671	471	285	159	82	55	11

[1]From Tables A and G of *Cohort Fertility: Native White Women in the United States.*

TABLE 4

FINAL CUMULATIVE BIRTH RATES, BY ORDER OF BIRTH, FOR THE COHORTS OF 1915–19 AND 1920–24, ESTIMATED FROM THE RATIOS BETWEEN THE RATES FOR THESE COHORTS AND THOSE OF 1910–14 ON JANUARY 1, 1951

Cohorts of	Age Group	Total Births	Order of Birth						
			1st	2nd	3rd	4th	5th	6th & 7th	8th & Higher
			Ratio to Cumulative Rates of Cohorts of 1910–14[1]						
a. 1915–19	32–36	1.094	1.080	1.146	1.150	1.067	.976	.901	.875
b. 1920–24	27–31	1.283	1.247	1.411	1.322	1.125	1.024	1.038	1.000
			Estimates of Final Rates Based on Foregoing Ratios						
c. 1915–19	47	2,644[2]	872[3]	739	448	245	136	124	80
d. 1920–24	47	3,067[2]	1,006[4]	910[3]	516	259	142	143	91
			Per Cent Change from Rates for 1910–14 Cohorts						
e. 1915–19	47	8.3	8.0	14.6	15.0	6.7	—2.4	—9.9	—12.5
f. 1920–24	47	25.7	24.7	41.1	32.2	12.5	2.4	3.8	0.0

[1]Computed from Table 3.
[2]The estimates for all births are obtained by adding those for the various birth orders.
[3]Too high to be probable.
[4]Impossibly high.

of the cohorts of 1920–24 and 1910–14 on the basis of their cumulative rates up to ages 27–31 (reached by the later cohorts on January 1, 1951) soon runs into difficulties. Thus, the cumulative rates by these ages for the 1920–24 group exceed those of the earlier group by 24.7 per cent for first births and 41.1 per cent for second. (See Table 3, Table 4 and Fig. 3) Applying these percentages to the "medium" estimates of final rates of the 1910–14 cohorts (800 for first births and 632 for second) gives corresponding estimates for the 1920–24 group of 1,006 for first births and 910 for second. For 1,000 women to have 1,006 first births is obviously impossible. Since some women do not marry, some married couples cannot have any children, and others cannot have more than one, a first birth rate of 880 or a second birth rate of

830 are unlikely to be attained. More reasonable "high" estimates now appear to be 846 and 716. Combining these rates with those for other birth orders on line *d* of Table 4 gives 2,713 as a high estimate of the final rate for all births to the 1920–24 cohorts. This is about 11 per cent larger than the corresponding rate for the 1910–14 group.

It is quite possible, of course, that the relatively large rates for first and second births to the 1920–24 women by ages 27–31 reflect a tendency to start families younger in greater degree than was assumed in making the high estimates. The writer believes that reasonable "low" estimates for final first and second birth rates are 818 and 662, respectively. Making corresponding allowances for other birth orders gives 2,362 as the "low" estimate for the final rate for all births. This would be a gain of about one per cent over the comparable rate for the 1910–14 cohorts, but a decline of one per cent from that of the 1915–19 group.[5]

The larger cumulative rate by ages 22–26 of the 1925–29 cohorts (by January 1, 1951) than of the 1910–14 group (by January 1, 1936) is due almost entirely to the rise for first and second births. This is not surprising, for relatively few third or higher order births occur by these ages. A similar statement holds true to an even greater degree for comparisons between the cumulative rate for all births by ages 17–21 of the 1930–34 cohorts (240) and of the 1910–14 group (177). In both cases it is clear that relatively more young women have borne a first, a second, and a third child during recent years than during prior years.

The postwar tendency for more women to start childbearing while young is in part a natural result of the changes which have taken place in age at first marriage. The proportion of ever married women among those aged 15–19, 20–24, and 25–29 was (on the whole) slightly higher in successive census years from 1890 to 1930, but slightly lower in 1940 than in 1930 (probably because some of the marriages postponed during the severe depression in the early 1930's had not been made up). During the 1940's the upward trend was greatly accelerated; the censuses of 1940 and 1950 show the per cent married rising from 10.9 to 16.5 at ages 15–19, from 51.5 to 67.5 at ages 20–24, and from 76.8 to 86.8 at ages 25–29. Not all of the increase in cumulative first birth rates can be attributed to younger marriage, however. For example, the proportion of the women aged 20–24 who were married rose 31 per cent from 1940 to 1950, but the proportion of the women aged 21–25 who had borne a child rose 40 per cent from 1941 to 1951. Similarly, among women aged 25–29 the proportion married was 13 per cent larger in 1950 than in 1940, but the proportion with one or more children a year later was up 26 per cent — nearly twice as much.[6]

The rapid rise in the proportion of women marrying at younger ages, and in the proportion of brides starting their families with little delay after marriage, has been interpreted by some as practically proving that there will be a substantial increase in the size of completed families. It is obvious that the younger the age at which marriage occurs (although perhaps not younger than 16) the fewer the years of the usual reproductive period which will have passed before marriage. If no net change were to take place in other conditions affecting fertility, this alone would tend to raise average family size. The extent to which the increases which occurred between 1940 and 1950 in the proportions married at younger ages might be expected to increase final fertility may be partially evaluated by computing the average age at first marriage from the census data for

[5]The high, medium, and low fertility assumptions, and the material on which they are based, are discussed in detail in Chapter 7, Sections B and C, of *Cohort Fertility: Native White Women in the United States.*

[6]The differences between the percentage increase for marriages and that for first births are nearly as large if a lag of two years is used instead of one.

these years. The 1940 data give 21.7 as the median age at first marriage for white women, and the 1950 data 20.2. If the 47th birthday is taken as the end of the childbearing period, this indicates that the period has been lengthened from 25.3 to 26.8 years, an increase of 5.9 per cent. It may be suggested, of course, that the proportions of women married at the younger ages may increase during the 1950's and 1960's as they did during the 1940's. This would lift the per cent married at ages 15–19 from 16.5 in 1950 to 22.1 in 1960 and 27.7 in 1970 (both are quite possible), and the per cent married at ages 20–24 from 67.5 to 83.5 and 99.5 (the latter is practically impossible). The per cent married at ages 25–29, which rose from 76.8 in 1940 to 86.8 in 1950, would reach an upper limit in the late 1950's or early 1960's. It is highly unlikely, therefore, that earlier marriage will lengthen the average interval from marriage to the end of the childbearing period by as much as 15 per cent.

The foregoing procedure undoubtedly understates the influence which younger marriage would have on fertility in the absence of other changes, because it makes no allowance for the higher fecundity of women in the early than in the later portions of the childbearing period. It should be remembered, however, that fecundity diminishes during this period not only because of aging or other variations associated merely with time, but also because of complications connected with the first, the second, or a subsequent pregnancy. Unfortunately, so little information is available on these matters that there is little basis for trying to evaluate them quantitatively.

A second reason which is advanced for believing that marrying younger will lead to substantially larger families is based on results from many studies of fertility differentials between geographic, economic, and social groups. These show a high inverse relation between the age of marriage and the fertility of the groups investigated. Unfortunately, little evidence has been collected as to the extent and effectiveness of family planning in such groups. It may well be that differences in this variable rather than differences in age at marriage are responsible for most of the fertility differentials in question.

While younger marriage in itself, no doubt, will tend to cause increases in size of completed family, there will be influences working in the opposite direction. Among these are the more general diffusion of information regarding the better methods of planning family size which are now available, and the development of still more effective methods. Indirect evidence points conclusively to the fact that an important proportion of couples have had larger families than they wanted in past decades, but that this proportion has been decreasing over time. Among the native white Protestant couples included in the Indianapolis study, approximately 80 per cent had no more children than they wanted and 20 per cent an excess number.[7] The proportion of couples who were successful planners in 1941 undoubtedly was substantially higher in this Indianapolis group than in the entire United States. Now it almost certainly is larger in both groups than it was in 1941; it can increase appreciably in the future.

The crucial question with respect to future trends in size of completed family, therefore, is: What changes will take place in the number of children wanted? Opinions on this subject differ widely and cannot be adequately supported with facts. The study in Indianapolis is one of the few in which statistical information was gath-

[7]The information for "relatively fecund" couples comes from P. K. Whelpton and Clyde V. Kiser, "Social and Psychological Factors Affecting Fertility, VI. The Planning of Fertility," *The Milbank Memorial Fund Quarterly*, Vol. XXV, No. 1, January 1947, page 86 (reprint page 111); that for "relatively sterile" couples comes from P. K. Whelpton and Clyde V. Kiser, "Social and Psychological Factors Affecting Fertility, VIII. The Comparative Influence on Fertility of Contraception and Impairments of Fecundity," *The Milbank Memorial Fund Quarterly*, Vol. XXVI, No. 2, April 1948, page 208 (reprint page 329).

ered. The average number of children wanted by the 1,977 couples included was 2.2, but whether this is smaller or larger than the corresponding number for similar couples in earlier or later years is not known. More is known about changes in opinions as to the "ideal" number of children per family, thanks to national polls by the American Institute of Public Opinion in 1941 and 1945. Averaging the replies of women aged 21–34 gave 3.0 children as the "ideal" in 1941 and 3.3 in 1945. The only later survey of a similar nature known to the writer relates to a sample of ever married Detroit women 40 years of age or older who were interviewed in 1952.[8] Averaging their replies gives 3.3 as the "ideal," which is practically the same as the national figure for 1945. Since it would be expected that women in a large city like Detroit would tend to favor smaller families than those in smaller cities, in suburban areas, and on farms, it is probable that a national poll in 1954 would elicit a larger "ideal" than that of 1945.

In comparing the foregoing "ideals" with the number of children borne per 1,000 women in actual cohorts, it must be remembered that the "ideals" make no allowance for women who remain single, nor for couples who cannot have a child.[9] It should also be remembered that "ideals" tend to exceed performance. Thus, the Detroit women whose replies gave 3.3 children as the "ideal" number had had only 2.6 themselves.

The forecasts of future fertility presented by the writer in the monograph mentioned above are based on three sets of assumptions made two years ago as to what will follow the reversal of the downward trend of the final cumulative birth rate. The high assumption is that there will be an upward trend which will lead to a rate of 2,800 for the cohorts of 1945–49, a gain of about 500 (or 22 per cent) from the low mark of about 2,300 which will shortly be set by the 1909 women. At the other extreme, the low assumption is that the rise will be of short duration, will reach a peak of about 2,400 for the cohorts of 1915–19, and will be followed by a decline which will reduce the rate to 2,000 for the cohorts of 1945–49 and later years. The medium assumption is for a rise to about 2,550 for the cohorts of 1920–24, and then a gradual decrease to 2,400 for the cohorts of 1945–49 and later. Space does not permit a discussion of the relation between these assumptions and the past trends of the cumulative rates of actual cohorts or the poll information as to "ideal" number of children.[10] The writer can merely repeat his belief that the actual rates will lie within the high and low extremes, and more likely above than below the medium series.

III. Implications for Annual Fertility Rates

This section will be devoted to the trends in annual birth rates up to 1965 which would occur if the changes in average size of completed family should follow the high, medium, or low assumptions discussed above.

If, in accordance with the high fertility assumption, the final cumulative rate for all births rises from about 2,300 for the women in cohort of 1909 (by January 1, 1956) to 2,800 for those in the cohorts of 1945–49 (who will start bearing children in the early 1960's), can it be taken for granted that the annual rate for all births will rise also? A partial answer to this question may be obtained by examining the data in Table 5. Line *h* of this table shows the final cumulative birth rates for a hypothetical group of cohorts living through the childbearing period under the

[8]Ronald Freedman and Harry Sharp, "Correlates of Values about Ideal Family Size in the Detroit Metropolitan Area," an unpublished manuscript.

[9]For a more detailed discussion of this topic and of the national polls, see *Cohort Fertility: Native White Women in the United States*, Chapter 7, Subsection 4.

[10]For such a discussion see Chapter 7, Section C, of the monograph.

current fertility conditions of 1945–49, *i.e.*, having during various age intervals the age specific birth rates which actual cohorts had at these ages during 1945–49 (shown on lines *a* to *g*).[11] Corresponding rates for 1950–54 are shown on lines *i* to *p*; these are estimated from the numbers of births by age order during 1950 and of all births the actual cohorts of 1945–49, almost all of the excess occurring with first and second births. The process gives obviously impossible results for first births — rates of 1,078 and 1,011 first births per 1,000 women — which emphasizes the fact that the high age specific first birth rates of 1945–49 and 1950–54 were a temporary phenomena

TABLE 5

FINAL CUMULATIVE BIRTH RATES, BY ORDER OF BIRTH, FOR HYPOTHETICAL COHORTS WITH AGE SPECIFIC RATES OF 1945–49 AND 1950–54, AND FOR ACTUAL COHORTS OF 1945–49 ACCORDING TO MEDIUM FERTILITY ASSUMPTIONS

Age Interval	*Total Births*	*Order of Birth*						
		1st	2nd	3rd	4th	5th	6th & 7th	8th & Higher
		Hypothetical Cohorts with Age Specific Rates of 1945–49[1]						
a. Up to 16–20	136	109	24	3	—	—	—	—
b. 16–20 to 21–25	729	425	212	66	19	5	2	—
c. 21–25 to 26–30	881	324	295	147	65	29	18	3
d. 26–30 to 31–35	627	144	190	127	72	42	38	14
e. 31–35 to 36–40	382	58	91	77	50	33	40	33
f. 36–40 to 41–45	159	16	26	26	20	15	22	34
g. 41–45 and older	26	2	2	3	3	2	3	11
h. Total	2,940	1,078	840	449	229	126	123	95
		Hypothetical Cohorts with Estimated Age Specific Rates of 1950–54						
i. Up to 16–20	204	158	42	4	—	—	—	—
j. 16–20 to 21–25	891	474	268	106	31	8	4	—
k. 21–25 to 26–30	926	262	317	192	90	39	22	4
l. 26–30 to 31–35	599	71	153	169	99	50	42	15
m. 31–35 to 36–40	389	28	80	101	59	44	48	29
n. 36–40 to 41–45	192	14	40	34	31	20	25	28
o. 41–45 and older	39	4	7	5	5	5	5	8
p. Total	3,240	1,011	907	611	315	166	146	84
		Actual Cohorts of 1945–49 High Fertility Assumption						
q. Total	2,800	850	720	510	310	180	155	75

[1]The procedure here is similar to that used in computing the conventional gross reproduction rate. The rates on line *a* are the cumulative rates of the 1930–34 cohorts by January 1, 1950. Those on line *b* are the additions to the cumulative rates of the 1925–29 cohorts from January 1, 1945 to January 1, 1950, etc.

from January, 1951, to August, 1953. Finally, line *q* shows the cumulative rates by age 47 for the actual cohorts of 1945–49 according to the high fertility assumptions.

Putting together the birth rates of successive cohorts at successive ages during 1945–49 and 1950–54 gives hypothetical final cumulative birth rates of 2,940 and 3,240, respectively. Both of these figures exceed the "high" assumption of 2,800 for which could not continue for many years. That they occurred at all was due partly to the making up in these periods of first births postponed during World War II (and perhaps even earlier), and in greater degree to the starting of families then, rather than later, by the hundreds of thousands of couples who married at younger ages than was customary with pre-war marriage patterns.

Rates for second births during 1945–49 and 1950–54 were improbably, rather than impossibly, high from a longtime stand-

[11]The procedure used here is equivalent to that used in computing conventional gross reproduction rates, based on age specific birth rates.

point. The numbers of these births during the two periods also were swelled by the bearing of the second babies postponed from prior years, and those advanced from later years as a result of more marriages at younger ages. Due to the time which elapses between marriage and the first birth, and between successive births, the tendency to marry younger did not affect third birth rates appreciably until the current period, and will not affect fourth and higher order births until later.

Looking ahead to 1955–59 and 1960–64 it is certain that even if the average size of completed families increases in accordance with the high assumptions, first birth rates will be substantially lower than during 1950–54, continuing the decline which went on from 1945–49 to 1950–54. Rates for second births also will decrease, in this case from the high mark of 1950–54. Third birth rates may continue to be large for another five-year period, but are certain to be smaller in 1960–64. With these births, therefore, the question is not whether the rates will rise or fall but whether the reduction will be small (as in the high assumptions) or considerably larger. Increases may occur with higher order births, however. The high assumptions raise the rates for fourth, sixth and seventh births during 1955–59 and 1960–64 slightly above the 1950–54 figures; for fifth births the upswing is substantially larger. Rates for eighth and subsequent births are shown as slightly higher during 1955–59 than 1950–54, but as declining thereafter.

The rates for all births according to the high assumptions are 3,151 for 1955–59 and 2,987 for 1960–64; they represent decreases of 2.7 and 7.8 per cent, respectively, from the rate of 3,240 for 1950–54. Since the high assumptions allow not only for an increase of nearly 20 per cent in size of completed families from the cohorts of 1905–09 to those of 1945–49, but also for substantial increases in the proportion of women who marry and start childbearing at the younger ages, it seems quite unlikely to the writer that the actual birth rates during 1955–59 and 1960–64 will exceed those in the high series. In other words, a decline in annual fertility rates, starting within two or three years and lasting about ten years, can be expected with a high degree of certainty. It may be small in accordance with the high assumptions outlined above, but it may also be large in accordance with the low assumptions to be discussed next.

The low fertility assumptions are based on a minor reversal of the past downward trend of final fertility rates (a rise from 2,325 for the cohorts of 1905–09 to 2,393 for those of 1915–19), and then a slow decrease to 2,000 for the cohorts of 1945–49. In addition, they allow for a reversal of the trend toward younger marriage and childbearing, and a return to the patterns of the early 1940's and the 1920's, but not those of the depression years of the 1930's. Such a trend could begin easily if the economic situation should worsen substantially and there should be corresponding changes in other factors influencing age at marriage. Under these conditions there would be a large scale postponement of marriage, and of the starting of families by married couples or the adding of children to the families already begun. Even with a much less severe depression than that of the 1930's, there would be reasons for expecting larger reductions in annual fertility from 1950–54 to 1955–59 than those which took place from 1925–29 to 1930–34. The most important reason is that the tendency to marry and start families younger has been several times greater during the last ten years than during the 1920's; hence, there has been a correspondingly greater advancement of births from 1954 or later to 1953 or earlier than there was from the 1930's to the 1920's.

The meaning of the low assumptions in terms of annual fertility during coming years may be seen by utilizing the same procedure as for the high assumptions, namely, combining the estimated rates for successive actual cohorts at successive ages.

The results are a final cumulative birth rate of 2,146 for 1955–59 and 1,780 for 1960–64. These are barely two-thirds as large as the corresponding rates of 3,151 and 2,987 for the high fertility assumptions, and represent declines of almost 34 and 45 per cent, respectively, from the rate of 3,240 for 1950–54.

Substantial differences are found between the low and high assumptions for births of each order, with a tendency for them to become somewhat larger relatively at the higher birth orders. The differences for the lower order births reflect primarily the assumptions regarding the timing of births — the continuation of the recent tendency toward childbearing at younger ages in the high assumptions, and the reversal of this tendency in the low. The differences for the middle and higher order births reflect in increasing degree the assumptions regarding changes in final fertility rates. The reason is that the rise of the final rate to 2,800 for all births to the cohorts of 1945–49 in the high assumptions is based on the following changes from the cohorts of 1905–09: a relatively small increase (9 per cent) for first births, successively larger increases for second, third, and fourth births (reaching 40 per cent for the latter), smaller increases for sixth and seventh births, and a decrease of 29 per cent for eighth and subsequent births combined. In contrast, the rate of 2,000 for all births to the 1945–49 cohorts in the low assumptions is based on very slight decreases (one per cent) from the first and second birth rates of the 1905–09 cohorts and successively larger reductions at subsequent birth orders (amounting to about 85 per cent for eighth and later births combined).

The medium assumptions are midway between the high and the low. They are based on (a) an increase in the final fertility rate from 2,337 for the cohorts of 1905–09 to about 2,550 for those of 1925–29, and then a gradual decrease to 2,400 for the 1945–49 group, and (b) a slight decrease from the present record-breaking, high proportions of women marrying at the younger ages. Under these conditions the age specific rates of successive cohorts during 1955–59 and 1960–64 would add up to final hypothetical rates of 2,647 and 2,386, respectively. These represent reductions of about 18 and 26 per cent from the corresponding rate of 3,240 for 1950–54.

In January, 1954, with many saying that an economic "readjustment" is underway but that there will not be a real depression in the near future, it seems unlikely that fertility rates will be close to the low assumptions during the next five or ten years, and probable that they will be above the medium. Nevertheless, it is well to remember that when family planning is as widespread and effective as it now is, annual fertility can change rapidly. The writer has not heard of anyone seriously interested in population questions who wrote between 1935 and 1945 that the 1946–54 period probably would see the high fertility rates and large numbers of births which have occurred. On the contrary, as J. S. Davis has emphasized, users of the population forecasts which the writer helped to prepare during 1930–1943 clearly preferred the medium or even the low fertility assumptions to those of high fertility, although the latter were substantially below the actual fertility of 1945–54. Because it still is so easy to be unduly influenced by the recent situation when thinking of what may happen in the future, the writer feels strongly that too little consideration is being given to the possibility that a major, rather than a minor, downward shift in annual fertility rates will occur within a few years.

Will the decrease in annual birth rates during the next ten years (or thereabouts) bring a reduction in annual numbers of births, or will it be more than balanced by population growth? In this connection it should be remembered that, although the total number of persons has been increasing rapidly during recent years, the number of women at some of the childbearing

ages not only has been decreasing but will continue to decrease for a time, because of the substantial decline in the annual number of births that went on from 1921 to 1933. As a consequence of that change there will be a somewhat larger reduction from 1950–54 to 1955–59 for the annual number of births than for the annual birth rate. In contrast, the increase in the number of births after 1933 will soon have a greater influence than the previous decrease, and will tend to raise rather than lower the number of births from 1955–59 to 1960–64. In consequence, the low assumptions show a smaller decrease between these two periods for the number of births than for the annual birth rate, and the high assumptions show a slight increase in the number of births in spite of the decrease in rates.

The foregoing discussion relates to changes in the fertility of native white women. As mentioned earlier it should apply to the changes for all women fairly well because such a large proportion of them are native white. Allowances are needed, however, for the influence of the more widespread and effective control of fertility by native white women than by others during past and future years. One consequence of this differential is that the changes in the timing of births have been, and for some years will continue to be, relatively larger among native white women than others. Another is that the downward trend in average size of completed family is not likely to stop at as early a date for other women as for native white women. A third is that there may not be a subsequent increase in average family size for other women, and if one does occur it almost certainly will be smaller than that for the native white group. Finally, since native white women have tended to marry at slightly older ages than other women, the recent tendency to marry younger very probably has been less important for the other women, and has caused less advancing of births from 1954 or later to 1953 or earlier. This will tend to support the annual fertility rates and numbers of births of foreign-born white and colored women in slightly greater degree than those of native white women during the next decade.

Statements about probable fertility trends after 1965 are necessarily more speculative than those for intervening years. All that will be said here is that both the low and high assumptions show higher rates for 1965–69 than 1960–64. This occurs with the high assumptions because they are predicated on an upward trend in average size of family, some continuation of the tendency to marry younger, and no depression or other change which will cause births to be postponed. It occurs with the low assumptions, in spite of the decline in family size, because of the making up of the births which in this series are shown as postponed during 1955–64.

HEREDITY OF JOINT DISEASES

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THE PROBLEMS of arthritis are gradually commanding a larger portion of public attention due in part to the educational activities of the Arthritis and Rheumatism Foundation, the establishing of new arthritic clinics throughout the nation, the training of increasing numbers of specialists in this field and the growing awareness by public and private welfare agencies of the personal, social, economic and family problems arising from this group of diseases. It seems therefore appropriate to discuss special phases of this medical problem with informed lay groups. One of the problems concerning which there is much misinformation is in the field of joint disease heredity.

The mistake is often made of considering all arthritis as one disease. Actually, many different diseases with different causes, characteristics and clinical manifestations have symptoms of the joints. Even the same type of arthritis differs in many respects as it involves different joints. Heredity may be a factor in producing arthritis, but its importance and the mechanism by which it works varies considerably from one disease to the next and for various combinations of joint involvement with the same disease. In some types of arthritis the influence of heredity and its mechanism of effect seems to be conclusively demonstrated; in other types it is strongly suspected, and in still others it seems to play little or no part.

Many different classifications of joint disease have been suggested, but most of them consider two main types of disease. These differ markedly in their clinical manifestations, tissue changes produced, the theory of their causes and the methods of their treatment. One of these diseases is osteoarthritis, a degenerative joint disease. This disease is considered to be the result of a wearing-out process, a premature aging of the tissues, or a degenerative process. As one would expect, it is essentially a disease of middle or later life, but it also tends to involve joints which have been injured, are malformed or are subject to excessive use. In osteoarthritis the patient is well and his general health is good; only the joints seem to be affected.

The heredity of Heberden's nodes, or osteoarthritis of the fingers, will be discussed first because the influence of heredity in this condition has been clearly demonstrated. Heberden's nodes are enlargements of the end joints of the fingers. They are called Heberden's nodes after the man who described them briefly in 1799. They occur in two different forms. The first, called traumatic Heberden's nodes, occur immediately as an enlargement of the finger joint after injury. This is a severe, painful, single injury such as occurs from being hit by a baseball or a hammer. The condition reaches a resting stage in several weeks; it is much more common in men than in women. Heredity has nothing to do with this disease.

The second type, so-called idiopathic Heberden's nodes, starts for no known reason in the end joint of one finger and spreads gradually to others until nearly all are involved. It is ten times as common in women as in men; the incidence advances gradually from the second to the eighth decade. It is definitely a hereditary disease.

How do we know this? The reasoning resulting in this decision is as follows: The author saw a man with enlargement of all his fingers who said four sisters were similarly affected, a fact substantiated by examining them. The question arose as to whether or not this observation was signifi-

cant. Over 8000 individuals were seen in the hospital, public clinics and homes for the aged to obtain significant data on the incidence of finger enlargement in men and women and in different age groups. It was soon apparent that some enlargement was due to injury, some not, that women were affected with idiopathic Heberden's nodes much more frequently than men, and that the incidence increased as age advanced. The incidence of idiopathic Heberden's nodes was found to be about 30 per cent after the age of 70 in women. An intensive study of women was then pursued in which the mothers and sisters of affected women were examined. Since the incidence of the disease in various age groups was known, it was easy to determine how many affected people were to be expected in any group of known size and age and sex. It was found that the mothers of affected women had Heberden's nodes twice as commonly and the sisters three times as commonly as one would expect in similar groups from the population in general. Certain combinations of multiple involvement in families were not to be expected to occur on a basis of chance alone more often than once in 5,000,000 and once in 10,000,000 families. Heredity seemed to be the best explanation for these observations.

The mechanism of inheritance was then studied. The data, the computation and the conclusions have been reported in detail elsewhere, and since they are quite technical will be omitted here. Suffice it to say that Heberden's nodes is a single autosomal character, sex influenced to be dominant in women and recessive in men. Under these circumstances an affected woman would transmit the disease to one-half her daughters. One-half her sons would inherit the trait and would transmit it to one-half of their children although they themselves would not show the disease. Because the disease is recessive in men, a man would show the disease only if he inherited it from both parents. Such a man would transmit the disease to every daughter and the trait to all his sons. In a dominant trait one parent is expected to be affected. This is not always true of Heberden's nodes because the unaffected father who carries the trait can transmit it.

The onset of Heberden's nodes is related in some way to the menopause since one-half the patients first noted finger enlargement within three years of the last menstrual period. Of 99 women studied for these factors, it was found that the women with early onset of Heberden's nodes had early onset of menopause and the women with later onset of Heberden's nodes had later onset of the menopause. The average age of onset of Heberden's nodes is about 50, varying from 30 to 70.

Let us next consider osteoarthritis of the hip. This is a painful, deforming disease resulting in limitation of joint motion, lameness and deformity of the bones seen clinically, by radiological examination and proven by specimens seen in anatomical museums. The socket of the joint in the pelvis becomes enlarged and surrounded by a huge bony growth, the head of the thigh bone is enlarged and altered in shape and the neck of the thigh bone is deformed. It may occur in one hip or involve both. It seems to occur independently of osteoarthritis or of other joints. This disease is known to occur months or years after severe injury without hereditary influence. It occurs in patients who earlier had Legg-Calvé-Perthes disease or slipped epiphysis of the hip and also those with congenital dysplasia of the hip. Evidence is accumulating to indicate that Legg-Calvé-Perthes disease not only runs in families but seems to be inherited as a single factor recessive trait. Congenital dysplasia of the hip, an entirely different disease, seems at least to run in families, although whether or not it is hereditary and if so how it is inherited has not been clearly demonstrated. While osteoarthritis of the hip cannot be properly attributed to hereditary factors, certain cases occur as complications of hereditary

diseases. What proportions of these cases of familial or hereditary diseases, congenital dysplasia or Legg-Calvé-Perthes result in osteoarthritis is not known. Nor is it known what proportion of osteoarthritis is dependent upon such previous familial or hereditary factors.

Osteoarthritis of the spine, considered as another manifestation of osteoarthritis, seems to occur spontaneously or as a result of hard work or injury but entirely without recognizable hereditary influence.

Rheumatoid arthritis, the second great type of arthritis, is a general systemic disease causing its most marked effect in the joints. These become involved usually one after another over a period of months or years with pain, swelling, deformity and varying degrees of disability. Many different causes have been assigned to this disease, but none have been convincingly proven.

In a study of the genetics of rheumatoid arthritis the families of 224 patients, including 1667 individuals, were compared with the families of 488 controls, composed of 2759 individuals. Three per cent of all relatives of patients were found to have been affected compared to 0.6 per cent in the control series. Of relatives over the age of 50 years the figures were 5 per cent and 0.9 per cent. Rheumatoid arthritis is found among relatives of affected people 5 to 6 times as often as in people in general.

Among 224 families, 250 brothers and sisters had rheumatoid arthritis out of 1054 people, compared to 561 expected if the disease were inherited as a single factor dominant. Gene frequency computation, based upon 16 found affected among the 2759 in the control series, showed 0.58 per cent of the population picked at random with the disease. Since penetrance (the proportion of the population affected compared to the proportion bearing the genetic constitution to make the condition possible) was about 50 per cent, the gene frequency is doubled, 0.58 to 1.16 per cent. In other words, 1.16 per cent of the population has the proper genetic constitution for the disease and 50 per cent of this or 0.58 is affected. This indicates that 12 per 1000 are susceptible and about 6 per thousand affected.

Although heredity seems to play an important role in the course of rheumatoid arthritis, other secondary conditioning factors must also be significant. They have been sought repeatedly with only indifferent success. An extensive controlled study was pursued in England and published in 1950, in which 532 patients with rheumatoid arthritis were each paired with an unaffected person of the same sex, age and marital state. An arbitrary age of onset was assigned to each control person, the same as that of the affected person.

Data was gathered and related to the age of onset, real and simulated, as to tonsillitis, sinusitis, dental infections, accidents, exposure to inclement weather, faulty nutrition, unhygienic living and working conditions, pregnancy and its complications, menopause, social disaster, emotional stress and other pertinent factors. The only significant difference noted between the two groups seemed to be that the patients had a higher proportion of affected relatives than did the control group and heredity was the one single factor demonstrated.

Ankylosing spondylitis is a particular form of arthritis resulting in a stiffened back. It is less common than rheumatoid arthritis, affects men more frequently than women and comes in early adulthood. That it is a familial disease is indicated by the fact that involvement of twins, brothers, fathers and sons and other instances of multiple family involvement have been recorded. In a study of families of 50 spondylitis cases secondary cases were recorded in 7 of 297 relatives compared to 2 in 2932 relatives of a combined control series. It was shown that spondylitis is inherited as a single autosomal factor with 70 per cent penetrance in men and 10 per cent penetrance in women, with heterozygotes or sus-

ceptible people occurring in 6 out of 10,000 in the population.

Gout has been recognized as a hereditary disease, but the pattern of inheritance has not been clearly identified. It has long been known to be associated with high uric acid levels in the blood. With the report that significant numbers of relatives of gouty people had high levels of blood uric acid without joint symptoms of gout, it seemed logical to study the genetics of hyperuricemia rather than of gout itself. Serum uric acids were done on 136, all the available relatives of 44 gouty patients. Hyperuricemia was found in 2 of 11 mothers, 4 of 25 brothers, 5 of 24 sisters, 5 of 33 sons, a total of 16 of 136 relatives but never in 45 daughters. Considered as a simple dominant, dormant hyperuricemia was found to be inherited with penetrance of 84 per cent in men and 12 per cent in women.

Plasma urate levels have been found to be higher in men than in women. This is true of normal people, of gouty patients and of hyperuricemic relatives of gouty patients. Men who inherit hyperuricemia often do not show it until puberty. Women do not usually show it until after the menopause. There is some correlation between the duration and magnitude of hyperuricemia and the onset of gout. These factors all help to explain why clinical gout is so much more common in men than in women and why the incidence increases with age.

Wofson has recently advanced a new theory of gout. He believes that the inherited susceptibility to gout depends upon an abnormal male sex hormone from the adrenal which interferes with uric acid excretion by the kidney and thereby produces hyperuricemia. These abnormal male sex hormones produce effects abnormally marked on uric acid metabolism but normal on masculinity. Attacks of clinical gout depend upon a second endocrine disturbance, an inability to produce promptly and in adequate amounts needed 11-oxysteroids during periods of stress due to failure of the pituitary to release adequate amounts of ACTH. This explains why ACTH is effective in relieving acute attacks of gouty arthritis.

Rheumatic fever is an acute inflammatory disease of the joints with fever. The patient invariably recovers completely from the joint symptoms, but the disease is a cause of serious, disabling, and at times fatal, heart disease. Rheumatic heart disease is second only to accidents as a cause of death in children and young adults. Acute attacks of rheumatic fever are always preceded by streptococcal infection although this is not recognized in nearly half the cases. When large groups of young people have been carefully observed, it has been found that only 3 per cent of people with streptococcal infection develop rheumatic fever. Such people with recognized preceding streptococcal infection account for 60 per cent of the cases of rheumatic fever. It is easy to see that individual susceptibility is very significant.

Convincing evidence is available which indicates that rheumatic fever is inherited as a simple recessive character. This means that children of two affected parents would all be susceptible to the disease; of one affected parent and one carrier, one-half would be susceptible; of one affected parent and a non-carrier none would be susceptible; of two carrier but not affected parents one-fourth would be susceptible. Such proportions would be seen only in dense populations where streptococcal infections were endemic as in slum conditions in New York. Children would probably avoid the disease regardless of their parents' history and their own susceptibility if they lived in Puerto Rico, Florida or Arizona, regions where streptococcal infections are rare or non-existent. They would also avoid the disease in New York if protected from streptococcal infections by prophylactic doses of penicillin or sulfa drugs.

The above brief survey of arthritis proves that joint symptoms arise from different

diseases, different even though they may bear the same name. Inheritance varies considerably from one disease to the next, being dominant in most, recessive in several others. The sex incidence, the age of onset, the penetrance and the gene frequency all show marked variation from one disease to the next. The relative importance of secondary causative factors acting upon susceptible individuals varies. Finally the evidence of heredity varies from being conclusive to being lacking entirely.

Review of the evidence presented here allows certain interesting conclusions concerning therapy and prevention. It seems unlikely that any of the diseases listed above occur with sufficient frequency or cause sufficient amount of disability or dependency to justify serious efforts at control by eugenics or sterilization. Less drastic methods may become useful.

The onset of Heberden's nodes is late in middle life, and as the expectancy of life and the average age increases the population promises to include a higher proportion of elderly and therefore affected individuals. The onset in women seems to be related to the menopause. If the menopause could be prevented or delayed or the chemical and physiological effects of the menopause could be prevented or properly counteracted, the onset of Heberden's nodes could conceivably be delayed or even prevented entirely. The necessary knowledge to accomplish this result does not seem to be available at present.

Some cases of osteoarthritis of the hip develop in deformities caused by diseases which are possibly hereditary or familial, Legg-Calvé-Perthes disease and congenital dysplasia of the hip. If these conditions are promptly recognized in children and properly treated, deformities of the hip are minimized or prevented and the probability of arthritis occurring is reduced. The author has no suggestion concerning osteoarthritis of the spine.

Rheumatoid arthritis presents a nice theoretical problem. It was pointed out that gene frequency and penetrance are both low indicating that a small proportion, 1.2 per cent, of the population is susceptible and only one-half of that succumbs to the disease. Possible secondary causative factors such as infected teeth, tonsils and sinuses, unhygienic living and working conditions, emotional, financial and personal stress, all part of the pattern of everyday living, which 98 per cent of the population sustain without developing rheumatoid arthritis, may well prove on closer examination to be critical to susceptible individuals. Much of the evidence on the cause of rheumatoid arthritis may have to be reevaluated and certain currently accepted conclusions modified.

The familial nature of ankylosing spondylitis is so well known that certain alert rheumatologists now routinely make X-ray examinations of the backs of all brothers of affected people. A surprising number of early cases have been discovered and treated. Even if the course of the disease cannot always be turned, deformities can be minimized or prevented and many of the devastating effects of the disease avoided.

Better understanding of gout based in part on genetic studies allows prompter diagnosis and effective treatment started earlier.

Much of the information on rheumatic fever presented here is under dispute, but prophylactic treatment of patients to prevent recurrent attacks and treatment of brothers and sisters of patients has decreased the incidence of this disease. Other factors such as better housing conditions, better nutrition, closer medical supervision of school children, closer check of men in the army and navy have played an important role in controlling the disease. Rheumatic fever now seems to be a controllable disease.

The above short and incomplete review indicates that genetic studies of arthritic diseases are important and may play a significant role in the handling of disease.

FAMILY ALLOWANCES IN GREAT BRITAIN, CANADA, AUSTRALIA AND NEW ZEALAND

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FOR THE MOST PART the benefits paid under the various Social Security schemes which have developed so rapidly in the past 25 years are intended to meet hardship arising from an emergency situation or a physical handicap or both; sickness allowances, old age pensions, unemployment benefits or compensation for industrial injury all fit into this framework. A partial exception may be found where maternity allowances form part of social security; childbirth and its attendant expenses are a normal contingency for most married people. But in many plans of this kind a needs test is imposed before maternity grants or allowances are given, thus bringing them again within the scope of hardship.

Family allowances, however, in Great Britain, Canada, Australia and New Zealand are in a different category; they are given in these countries to all families with dependent children (though as will be seen the first child is sometimes excluded) without a needs test and without restriction as to class, status, occupation or, generally speaking, race. Normally a residence qualification only is required as a condition for receiving family allowances.

In some European countries, these allowances form part of a national population policy, as an encouragement to parents to have children. This was of course notably the case in France which was the pioneer in the field of family allowances and which initiated and extended them as a means of combating a disquietingly low and continually falling birth rate; Nazi Germany and Fascist Italy followed the same policy, inspired partly by military considerations. It is debatable how far the aim of raising the birth rate, at any rate eugenically, has been achieved by these measures, and in later schemes, such as those in Great Britain and the Dominions, population policy has played a negligible role in the formation and carrying out of schemes for family allowances. It has been claimed indeed by some writers that as economic standards rise families become smaller, and therefore allowances will be a factor tending to lower rather than to raise the birth rate. But it does not appear that in the countries under consideration there has been any marked effect one way or the other.

In industrial countries, a strong argument that family allowances should be given as a matter of social policy, quite apart from any considerations of population, was and is the change in the position of children. Since the last quarter of the 19th century, they have ceased to be an economic asset, as they formerly were, as legislation has progressively restricted or prohibited their employment in remunerative work. Now up to at least the age of 14 or 15 and often later, they make little or no contribution to the family exchequer, and this, coupled with a general rise in standards of living and an increase in social responsibility, means that a young family is a considerable economic liability to its parents.

In these countries universal family allowances are now an integral part of the social security plans, but compared with other benefits, for example old age pensions, they are of comparatively recent date. Family burdens were, however, recognised in other

social legislation; income tax reductions were allowed in all cases for family responsibilities; some gave mothers and children's allowances in certain circumstances of need, though these were partial measures and in no case covered the whole country, nor the whole population; others gave maternity grants in childbirth. In Great Britain such allowances formed part of the Poor Law until the inception of the National Insurance system, when they were gradually incorporated within it — though never completely. In Canada, Australia and New Zealand, where the Poor Law tradition was much less strong, allowances for needy dependents were included in the assistance or pension schemes. That the mere existence of a family might constitute an economic burden which could not be carried without hardship by many wage earners, and which should be lightened by the community in its own interests, as well as from humanitarian motives, was very slowly appreciated. Such provision as existed up to the second World War was mainly through services in kind: cheap or free meals for school children, cheap milk and nourishment for pregnant or nursing mothers, and sometimes free medical and nursing services for women in childbirth.

Great Britain

In Great Britain the campaign for family allowances is indissolubly linked with the name of Eleanor Rathbone. Her book "The Disinherited Family" was first published in 1926 and remained until 1944, when her aim was achieved, the Bible of the family allowances protagonists. In it she stressed the fact that the British wage system, whatever its foundation in economic theory, was based on the individual and took no real account of family needs. Thus the man with a family of three or four children, drawing the recognised wage for his job and grade, might be living in conditions of poverty, while his fellow worker who happened to be childless or a bachelor would be quite comfortable on the same income. The remedy for this, said Eleanor Rathbone, was not to raise the general wage level which would be economically impossible and would give the childless an "unearned increment" but to pay allowances in respect of some or all dependent children. For many years after the book was published these views gained little public support; Malthusian views on population still commanded a good deal of respect, and it was feared that to "subsidise" children would result in a rise in the birth rate among the poorest groups. The fact that France had recently adopted an extensive system of family allowances as a part of a policy to counteract the falling birth rate, strengthened this point of view. Organised labour was also for long opposed to family allowances, fearing their effect on wages, even if they were financed from public funds, and not as in France, by a levy on employers.

The economic depression of the inter-war years showed in many ways the part played by family burdens in producing poverty. Rowntree in his "Human Needs of Labour" and in the much fuller survey "Poverty and Progress" based on 1936 material, stressed the importance of large families or even three or four children as a cause of poverty. Sir John Boyd Orr (as he then was) in his "Food, Health and Income" published in 1936 observed that 40 per cent of the children in Great Britain were during some part of their childhood, living in families where the total income was too low to provide a satisfactory and adequate diet for all members of the family. Various other surveys, both of areas such as those of Bristol and Merseyside, and also of groups, such as the Pilgrim Trust report "Men Without Work" made the same point — the economic burden of more than one or two children, often where the father was fully employed, still more where the family were on unemployment benefit or assistance.

An increasingly favourable attitude towards family allowances developed during

the last war, partly though not altogether because, with a nation under arms, more and more families were in fact receiving allowances as service men's dependents; the finishing touch was given when Lord Beveridge in his famous report on "Social Insurance and the Allied Services" made the establishment of a system of family allowances one of the basic assumptions of his plan. The Government subsequently announced its intention of setting up such a system, and in 1944 the Family Allowances Act was passed.

This has been amended in details as the cost of living rose, and the present scheme provides for a flat-rate weekly allowance (at present 8/–) for every child except the first until the age of 16; thus in a "sample" family of four children all under 16, three will draw allowances till the eldest becomes 16; then the second child having become the first under 16 ceases to draw, and so on. The allowances are paid to the mother and are subject to Income Tax, but not to any form of needs test. The scheme is administered by the Ministry of National Insurance.

In addition to these allowances Great Britain has a system of maternity grants and allowances paid to insured women or the wives of insured men, that is, to nearly every mother in the country. From October 1953, these will consist of a maternity grant of £9 payable shortly before or after confinement which may be supplemented by a further £3 if the woman is confined at home, and of a maternity allowance of 32/6 weekly for eighteen weeks payable to women who are employed and cease work owing to pregnancy. Unlike family allowances these benefits are conditional on a minimum number of contributions being paid under the insurance scheme.

Canada

In Canada as in Great Britain family allowances were a late development. One of the main difficulties in this as in other social security measures was a constitutional one concerning the powers of the Federal and Provincial legislatures. Already, before the Unemployment Insurance Act was passed, it had been necessary to amend the British North America Act so as to enable the Federal Parliament to carry through legislation of this kind, covering the whole Dominion and centrally financed, and when family allowances were first proposed, opposition was encountered, not to the principle — the necessity for easing family burdens was pretty generally recognised — but to the apparent encroachment on the rights of the Provinces.

The determinant factor in putting family allowances on the map was the publication of the Marsh Report. This as is well known was prepared for the Prime Minister's Advisory Committee on Reconstruction by Dr. L. C. Marsh, and was entitled "Report on Social Security for Canada; the Requirements for Post-War Planning." The report dealt with all aspects of social security, reviewed existing measures and suggested plans for coordination and extension, envisaging a plan to cover the whole field. One of the main recommendations was the introduction of family allowances, and in support of this Dr. Marsh attempted to define minimum standards of need and to show how many families and incomes fell short of them. By 1943 standards of family needs had been accepted by the Dependents Allowances Board of Canada for service men's families, and it was suggested that it was time that the principle should be extended to cover all families. The Marsh Report defines two standards; one taken from a study made by the Welfare Council of Toronto prepared in 1939, based on local needs and prices and adjusted to the 1941 level, was described as a "desirable living minimum" and allowed for a higher standard than, for example, Rowntree's human needs standard in Great Britain. This required monthly incomes of 17.85 dollars for a child, 69.9 dollars for a man and wife, and 122.88 dollars for a family of five. Dr. Marsh also defined a lower

standard, which he called an "assistance minimum" which would only be sufficient to maintain health over a short period and which allowed for no amenities. To reach this, the necessary income levels were 14.63 dollars for a child, 44.46 dollars for a man and wife, and 94.54 dollars for a family of five. On these requirements it was found (using the earnings data of the Dominion Bureau of Statistics) that for a family of five (three children) 33.4 per cent of the male heads of households in urban areas and 50 per cent in rural areas earned less than the assistance minimum, while the percentages falling below the desirable living minimum rose to two-thirds in urban areas and three-quarters in rural areas. Large families were only one factor in producing poverty, but, just as in Great Britain, they were an important one. The Report recommended the institution of a Federal scheme of children's allowances as part of a general social security plan, and after reviewing possible methods of administration and finance, advocated universal cash allowances, financed from general taxation.

Following on the Marsh Report, a special committee was appointed by the Canadian House of Commons to examine and report on a national plan of social insurance and this was followed by a Family Allowances Bill which became law in 1944. As already suggested, opposition to this was mainly on the Federal Provincial issue, but some individuals and groups were opposed to it on another point, namely, that by a federal scheme some provinces would benefit much more than others, although all would have to pay if the plan were financed by centrally imposed taxation. This is indeed the case, as is shown by the numbers of children who received allowances, which vary from 1,350,588 in 1950 in Quebec, to 33,588 in 1950 annually in Prince Edwards Island. Clearly such difficulties are inevitable in countries such as Canada where the area is vast and the population very unequally distributed between the provinces.

Eventually the provincial opposition was overcome, and a federal scheme of family allowances came into force in 1945, being administered by the newly established Department of Health and Welfare. As subsequently amended and liberalised, the Canadian scheme like the British gives allowances without any test of income, and goes further, in that family allowances do not form part of taxable income. All children — not excepting the first — under 16 and maintained by their parents or parent are eligible, and payments are based on a sliding scale according to the age of the child, ranging from five to eight dollars a month. In the original scheme the allowance was reduced where there were more than five children in a family, and there was a residential qualification of three years. In 1949 the former provision was withdrawn, and the latter amended to one year. The allowance is paid to Indian and Eskimo families, but for them the payment is made chiefly in kind. In 1951 nearly two million families with over four and one-quarter million children were covered by the scheme. A specific condition of the allowance is that the children attend school; payments may be withheld if this is not complied with, and if the authorities consider that the allowance is not being spent on behalf of the child it may be paid to some other person than the parents, or not given at all.

Mothers' allowances on a provincial basis are also awarded by all the provinces. These vary in amount, but all are based on need, and carry a residence and usually a nationality condition; to be eligible, claimants must usually be widows or have family incapable of maintaining the family—such incapacity includes desertion and sometimes divorce. In a few provinces, needy families who do not strictly fulfil these conditions may nonetheless be given allowances.

Australia

Australia presents some features that are similar to Canada, in that it has a federal constitution, and of course Commonwealth and State legislatures. It also has a wage structure which is controlled by Courts of Arbitration; wage-fixing Tribunals were established in Victoria in 1896 and in 1907 the first Commonwealth basic wage for unskilled workers was laid down, and described as being sufficient to maintain "an average man as a human being." Subsequent statutory basic wages have derived from this, and it was for long assumed that the standard laid down covered family needs. The existence of this statutory wage structure with its apparent family basis has been one of the obstacles to a Commonwealth scheme of family allowances. In 1926 a Royal Commission was appointed to enquire into the subject; the majority were not in favour of any action, nor was the Government of the day, affirming that the expense would be too great and that allowances would raise costs of production and of living so that no real benefit would result. However, various state schemes were in fact introduced during the succeeding years, based on a test of need.

By 1940 the tide of opinion had turned in favour of family allowances in Australia as in other countries of the British Commonwealth, helped by the war and the necessity for paying allowances to the dependents of Service men. A scheme of child endowment was promised in 1941 and was shortly afterwards introduced. During the course of the debates in the Commonwealth Parliament, the part which might be played by such a plan in population policy was discussed, and the need of Australia for more young people was stressed; counter arguments maintained that the Australian birth rate was already high and that any marked rise would be a disadvantage. But the Government declared that there was no reason to think that the scheme would have any great effect on the birth rate either way and the real reason for introducing it was the necessity for strengthening family life and lightening its economic burdens, which the basic wage no longer did. In former years, as one of the chief Opposition speakers pointed out, the problem had been to raise the status of labour; this was now achieved and the problem had changed to the disparity between the level of family life as laid down by Australian standards and the level attainable for many families on the statutory basic wage. It was also pointed out that the age of marriage was generally later in Australia than in Great Britain, thus increasing the economic burden of the larger families.

The scheme as originally introduced was to pay allowances to all dependent children except the first up to the age of 16, without a needs test, but with a residence qualification. At the present time the first child is also included, however, but receives a lower benefit — 5/– instead of 10/– a week, the latter being paid for subsequent children. The existing income tax deductions in respect of children were modified under the plan, so that the larger incomes contributed to the allowances of the worse off. Allowances in Australia are paid for children living in charitably financed institutions, and for those boarded out by the states, but not for those maintained in state-supported institutions.

The scheme is administered by the Ministry of Social Services and is financed by a payroll tax on amounts above £20 a week and by income tax.

Maternity allowances have been paid since 1911 to residents; these, which carry no test of need, take the form of a lump sum, varying according to the number of children in the family, and also a sum of £10, representing an allowance of 25/– a week for four weeks before and after childbirth.

New Zealand

Although its methods are the last to be discussed here, New Zealand took the lead

in giving family allowances, as it has been the pioneer in much social legislation afterwards adopted by other countries; for example, the first Old Age Pensions Act was passed in 1898.

Family allowances became established in New Zealand in 1927, when weekly payments of 2/– a week for each child were given for families with more than two children; they carried an income limit, at that time £5 a week, for the full allowance. Between 1927 and 1936 adverse economic conditions made it necessary to reduce the income limit, first to £3.12, and then to £3.5; but in 1936 it was again raised to £4. The Social Security Act of 1938 created a new Department of Social Security, which took over, among other duties, the administration of family allowances; the income limit was raised to £5 a week and the allowance to 4/– for each eligible child. In 1940 all children except the first were included, and in 1941 all children under 16. Further amendments in succeeding years increased allowances and raised the income limit, until in 1946, the Social Security Amendment Act (passed in the preceding year) abolished the income limit altogether, and included all adopted children as beneficiaries. The benefit remains at 10/– a week, and the scheme is financed by a special income and payroll tax.

New Zealand has no general scheme of maternity allowances; hospital treatment and midwifery services are provided free of cost, but no cash.

Conclusions

The various schemes of family allowances here discussed have one thing in common; they all give benefit without a needs test and without specific contributions as in social insurance. Otherwise they show considerable differences; Great Britain is alone in excepting the first child from its scope of benefit, although Australia gives a reduced allowance. Canada, Australia and New Zealand finance their schemes by special taxation, Great Britain does so mainly from general revenue, though the social insurance fund makes a contribution. Moreover, in Great Britain allowances are regarded as part of taxable income, while in the other countries they are not. In no case are the allowances large enough to meet the economic cost of bearing and rearing a child, and they have come increasingly short of this as the cost of living has risen, and allowances, while raised, have not kept pace with prices. While probably few people would urge that the community should in fact meet the whole cost of children, many critics have said that the allowances are too low. Canadian commentators on the Marsh Report and the subsequent legislation pointed out that benefits did not reach even the lower of the two minimum standards laid down in that document, and were too low substantially to ease the economic burden of children. Certain Australian representatives pointed out in the course of the various debates that allowances were sufficient to make a considerable contribution to the lower paid workers' budget but not enough to make much difference to the higher wage earner, and they urged that allowances should bear some relation to wages or salaries. It will be noted that none of the national schemes here considered recognised differential needs of this kind; all pay flat rates, except the Canadian plan, which varies according to the number of children, not according to variety in income. It is difficult to see how — even if it were desirable on general grounds — such differences could be recognised without levying a specific and differential tax.

In Great Britain the Royal Commission on Population, reporting in 1949, while admitting that Family Allowances had not been established as an instrument of population policy, pointed out that at the 5/– level which then operated they were a most inadequate alleviation of family burdens, and suggested that they should be raised to 12/– weekly, a suggestion which has not so far been fully carried out, the amended

allowances being 8/–. Criticisms have also been made though much less strongly of the wisdom or equity of paying allowances without income limit to all families; however, to impose a needs test would not only be administratively expensive and complicated but might discriminate against the thrifty and saving parents.

To conclude, it is reasonably certain that the demand for higher benefits will continue to "grow by what it feeds on" as costs and prices rise and higher standards in the welfare state are demanded, without any very clear idea as to how they are to be paid for or as to their relation to production. However, it is unlikely, in the opinion of the writer, that demands will be as insistent for family allowances as for, say, old age pensions. It is in the early days of welfare legislation that humanity and abstract justice are the moving springs; by now in the countries discussed, pressure groups of varying strength constitute the main motive force, though not of course the only one. Old age pensioners are a clearly defined, increasing and fairly well-organised body, who are beginning to form an important pressure group. But family burdens are much less easy to isolate, the heads of families are not an organised group and demand for higher family benefits is vague and ill-defined. It is more often vicariously voiced by social workers than by the people directly concerned, and in the legislative field altruism is only too often less effective than self-interest.

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GENETIC FACTORS AFFECTING INTELLIGENCE

*Symposium**

I. Heredity and Learning Ability in Infrahuman Mammals
II. Fertility and Intelligence Among Families of the Mentally Deficient
III. Intelligence in Family Groups in the Blue Ridge Mountains

FREDERICK OSBORN, *Chairman*

I. HEREDITY AND LEARNING ABILITY IN INFRAHUMAN MAMMALS

JOHN L. FULLER AND J. P. SCOTT
Roscoe B. Jackson Memorial Laboratory

INTRODUCTION

Scientific work on the relationship between heredity and human intelligence is by necessity largely descriptive. A great deal can be learned from studies on twins and other close relatives, but it is impossible to make experimental breeding tests of the theories which have been so obtained. Such work can, however, be done on other mammals, and it is the purpose of this paper to summarize the currently available information upon the subject.

At the outset we must recognize the limitation as well as the advantages of working with other species. It should be emphasized that findings on other animals cannot be directly applied to human beings, and it is only after careful study and comparison that we can draw even tentative conclusions about human nature. Certain special human problems cannot be studied in other animals, particularly those which are involved with the use of human language. For this and other reasons it is difficult to study animal intelligence in the same way that we study human intelligence. Part of our understanding of ourselves is based on subjective information which we can never get from animals, and this makes it necessary to use new terms as well as new methods. The dictionary gives seven or eight definitions of the word "intelligence" of which "the capacity for knowledge or understanding, particularly with reference to meeting novel situations" is perhaps the most scientific. Obviously, we can never tell whether an animal "knows" or "understands" a situation except by inference and, in studying animal behavior, it is better to use the more objective term of adaptation.

Here the biologist finds himself on familiar ground. From the time of Darwin the heritable nature of adaptive capacities has been one of the major tenets of the theory of evolution. In the study of

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fossil records adaptation can often be inferred from structural characteristics, but actual adaptation consists of the behavioral and physiological reactions of living animals. This is something which can be studied by direct experimental methods, and the results may uphold or modify basic biological theory as well as relate to the special problem of human intelligence.

Heredity could conceivably affect behavior and adaptation in one or both of two general ways. In the first place, it might provide capacities for developing special methods of adaptation which would be useful in situations which are repeated generation after generation. In the second place, heredity might provide an individual or a species with a set of capacities for adjusting to many different and variable situations which are not necessarily repeated generation after generation. From a theoretical point of view it would appear desirable for an individual to have both kinds of capacities.

The objective facts which can be observed in connection with each kind are, respectively (1) patterns of behavior and (2) modification of behavior in various ways from which capacities for learning can be inferred. The available evidence concerning these two kinds of facts will be analyzed in the following sections.

Differences Between Species

Patterns of Behavior

Comparative studies of the behavior of animals have shown that each species has particular ways in which it adapts to the environment. These patterns of behavior, which may include integrated activity of all parts of the body, are closely related to the sensory and motor abilities of the animal and they tend to become more complicated along with the sensory and motor apparatus. The assumptions of the biologists that structure is a good index of adaptation are therefore justified.

Considerable variety is seen in the patterns of behavior shown by any one species and a given environmental situation may evoke several reactions which tend to be used in a regular order. For example, if one mouse is attacked by another, it first fights back and, if this is unsuccessful, it starts to run away. If escape is impossible it may then stand still in a defensive posture or, in some situations, simply lie flat and passive in a manner which almost suggests death feigning (1).

It is seldom possible to test the inheritance of these patterns of behavior by experimental breeding as crossbreeding between most species is impossible. Evidence that behavior patterns are inherited comes from many species of lower vertebrates in which the parents never come into contact with the offspring and where biological heredity appears to be the only factor which could account for their persistence.

Tinbergen (2) has extensively studied the social reactions of lower vertebrates and finds that they are often evoked by very specific stimulation normally provided by some other member of the same species, so that an animal may be said to inherit the tendency to respond to a particular sign-stimulus. Relatively little work of this sort in mammals has been done, but the indications are that social behavior may be profoundly affected by a process of socialization (3) in which learning plays an important part.

The possibility of cultural as well as biological inheritance can be tested by raising animals either in complete isolation or in contact with a foster species with very different habits of behavior (4). The results of such experiments on birds have been described extensively by Lorenz who has found evidence of both cultural and biological factors. For example (5) jackdaws reared by hand gave the same reactions to human beings as they would ordinarily give to other jackdaws and showed no evidence of adopting human patterns of behavior. On the other hand, these same jackdaws showed very few avoidance re-

actions and would ordinarily not survive if released.

Similar results have been obtained by raising mammals apart from their own species, with perhaps more evidence of cultural inheritance. For example, the sheep (6) which is raised on the bottle becomes closely attached to human beings. Its characteristic patterns of vocalization, butting and sex behavior are the same as those of the native species. The following reaction may be developed toward people, but if not given the opportunity to do this the sheep becomes independent, neither avoiding nor closely following. Fear reactions do not appear to be biologically inherited, and a bottle-reared sheep described in an earlier paper was fearless of dogs and was later the only one killed when the flock was attached by these animals.

There is also some evidence based on naturalistic observations of species which have had the opportunity to change their habitats that certain mammals have a greater range of adaptability in behavior patterns than others. For example, the American elk was originally a plains-living animal. Later it was forced by hunters and ranchers back up into the mountains where it has adapted itself very well. Zoological parks report that these animals when taken from the wild adjust themselves to conditions of confinement very readily and are, to all intents and purposes, very tame after a few days. By contrast, the pronghorn antelope is never found in any environment except open plains and deserts and is very difficult to keep in confinement. One characteristic of these animals is their extremely speedy flight which is useful only in open country and which leads the animals to run into fences and injure themselves in captivity (7). Another case is that of the muskrat (8) which flourishes in any natural pond or stream but which has never yet been satisfactorily raised in captivity. Such instances could be multiplied indefinitely and they seem to be related to what the older naturalists called specialization, the possession of a trait or group of traits which made the animal highly adaptive under certain special conditions but made it nonadaptive in others.

In general, the evidence on interspecific differences in patterns of behavior indicates that the influence of biological heredity is very strong, particularly in basic patterns of social behavior. Individuals tend to develop the characteristic patterns of behavior of their respective species in spite of profound modification of the environment. At the same time much of the behavior of highly social species, where the young have many contacts with adults, may on experiment turn out to be largely determined by cultural heredity.

Learning Capacities

The general role of heredity in determining the learning capacities of a species may be experimentally tested in one way by drastically altering the environment. Hebb (9) found that albino rats raised in the "rich" environment of the author's home performed much better on mazes than those from the relatively "poor" environment of laboratory cages. Bernstein (10) found that such effects could be obtained simply by taking the rats out of their cage and handling them, indicating that the social environment was a very important factor as well as familiarity with the physical environment. The results of rearing chimpanzees in human homes (11, 12) can be interpreted in the same way.

At the same time biological heredity imposes definite limitations on learning capacities through sensory and reaction systems. Maier and Schneirla (13) have reviewed the animal kingdom from this point of view and find great variety in sensory and motor abilities even in mammals. The senses of vision, hearing and smell may be very acute or almost lacking in different species. A motor pattern like manipulation may be well developed in some mammals like raccoons and almost absent in the hoofed animals.

Another limitation is placed on learning ability by genetically controlled patterns of behavior. Experimenters need to be extremely careful in assuming that an animal lacks a general ability to learn since such ability can only be brought out within the range of adaptability of the species. There is no doubt but that a great many of the early experiments on animal behavior came to false conclusions by attempting experiments which did not fit the animal's capability. For example, maze tests are very good for bringing out adaptation in the rat which in the wild lives in a system of runways and tunnels. Similar tests are very poor for sheep which naturally live in the open and whose safety depends upon avoidance of narrow enclosures. A maze for sheep which simulated the rocky cliffs used by these animals might be effective where the rat type maze simply makes them extremely fearful and timid.

Some of these difficulties may be avoided by studying only the simplest type of learning. There is some evidence that the ability to associate consecutive stimuli is a fundamental property of nervous tissue throughout the metazoa and may even exist in protozoa. Within the vertebrates, Nissen (14), Razran (15) and others who have studied simple learning have come to the conclusion that all vertebrates are basically similar. Until recently it looked as if invertebrates were relatively poor at forming associations and that the learning was relatively impermanent. However, recent experiments by J. Z. Young (16) on the octopus, which has a rather highly developed motor apparatus and an extremely well developed sense of vision, have shown that these animals can be easily conditioned and give the same kind of experimental results as vertebrates. It would appear that differences in learning abilities are associated with differences in sensory and motor capacities and the question may be raised as to whether they exist independently.

An effort has been made by comparative psychologists to test this by using methods which make use of only simple sensory and motor capacities to solve complicated abstract problems. A great number of these experiments are suspect on the grounds that the tests used are unsuitable to the behavior patterns of the animal, that only one or two individuals were used in a species, and that the individuals used may not have been properly socialized and hence capable of being motivated. Nevertheless, such tests do appear to yield differential results.

Warden (17, 18) and Nissen (19) have described several kinds of tests which give species differences. One of these is the delayed response test, in which the animal reacts to a signal after a considerable delay and must discriminate between alternate signals. Another is the problem of single and double alternation of correct pathways which has been used in connection with mazes. In the multiple plate apparatus an animal has to learn to step on three plates in order and then reverse the order up to its limit of learning. These three tests are essentially memory problems which can be easily solved by the use of human verbal symbols. Another type of problem is one in which the animal has to make a choice from a number of possibilities, the only clue being that the last choice was never right. Harlow (20) has recently suggested an "oddity" test for primates in which the correct choice is always an object which is visually different from the other two.

All of these tests produce better performance by primates than other mammals and better performance by college sophomores than other primates. Starting with the somewhat prejudiced notion that primates are more intelligent than other animals, we may assume that these are tests of pure intelligence. It should be remembered that the monkeys have visual and manipulatory capacities which are closely similar to those of people, and the results may not be as independent of sensory and motor abilities as they seem.

It may be concluded that modifications

of the environment and particularly the social environment can produce important changes in learning capacities. Biological heredity produces great differences between species in learning capacities, operating through limitations of sensory and motor apparatus, as well as patterns of behavior. There is some evidence that certain abstract problems in which human and other primates excel are difficult for other mammals, but it has not been established that the ability to solve them is independent of the special visual and manipulatory capacities of primates.

Differences Within Species

Patterns of Behavior

Within a species there should be some variability in genetically controlled patterns of behavior or there would be no opportunity for evolutionary change. Considerable literature has accumulated along these lines and while much of it is imperfect in method, all of it is consistent with the assumption of genetic differences in patterns of behavior.

The most extensive work has been done with sex (which is of course biologically inherited) and its effect upon reproductive patterns of behavior. Present evidence indicates (21) that in mammals both sexes may exhibit the patterns of behavior which are ordinarily considered to be characteristic of one sex. Under ordinary conditions one reaction tends to be favored over the other because of anatomical limitations and hormonal alteration of thresholds of stimulation which are ultimately determined by the segregation of X and Y chromosomes in the male parent.

Recently Young and his co-workers have found genetic differences within the male sex (22). Inbred strains of guinea pigs consistently differ in sexual behavior, and particularly in sex drive or motivation, from a heterozygous stock.

Hall (23) had previously made important genetic studies on the inheritance of a non-sexual pattern of emotional behavior in the rat. Starting with a heterozygous stock he was able to select two separate strains of rats: one which defecated frequently when placed in an open area, and one which did not.

A large number of studies have been made on wildness and tameness, and fighting reactions of rats and mice (1). Most of these studies are connected with strain differences and there are very few on which good data on crossbreeding exist. Nevertheless, certain conclusions can be drawn. Wild rats and mice differ markedly from the tame strains, and the inbred strains of mice show large differences in fighting. Males are much more easily stimulated to fight than females. There is only one experiment which gives any clearcut genetic results since, in most cases, pure strains were not used in the crosses and complete experiments were not performed. The experiment of Dawson (27) is interesting in that he found that less than two genes would account for the differences in wildness and tameness which he tested.

At the present time Thompson and Fuller (24) are making a behavioral survey of inbred strains of mice and find wide differences in such traits as exploratory behavior. A program of crossbreeding is now under way.

Thus there is abundant evidence of the existence of genetic differences in patterns of behavior within species. These differences consist chiefly of variability in drives and emotions rather than any fundamental modification of the nature of the behavior patterns themselves.

Learning Capacities

Relatively few attempts have been made to study the variability of learning capacities within a species below the human level and one of the most ambitious of these was done by Tryon (25) in his experiment on the inheritance of maze-running ability in rats. Starting with a mixed stock of albino rats, he selected animals for good

and poor performances on a maze. This apparatus was entirely mechanical in nature so that human bias might be eliminated from the experiment, but the mechanism did make noise and caused other disturbances to the animals. After selection had gone on for several generations, almost complete separation was obtained between two groups of animals which were described as maze bright and maze dull. In a later experiment Tryon asked a graduate student to measure the ability of these animals in other mazes. Searle (26) tested the animals in thirty situations and found that the performance varied greatly on other pieces of apparatus and that the so-called dull animals performed better in about as many situations as did the so-called bright animals. He came to the conclusion that selection had largely operated through the emotional response of the rats, the dull animals being afraid of the maze and the bright ones not being alarmed by its mechanical clickings. The conclusion may be reached that the important genetic variability which existed in the stock was concerned with a pattern of behavior of timidity and its emotional causes rather than a direct modification of a general learning response.

At the present time Thompson (28) is attempting a similar experiment which involves selection for poor and good performance on the Hebb-Williams maze, which is nonmechanical in nature. Care is being taken to gentle the rats by handling before testing them. It is hoped that in this way selection will avoid fear reactions, and it will be interesting to see whether the differences obtained are less specific to the situation than in Tryon's work. However, at the present time the demonstrated intraspecific differences in learning capacities appear to be special in nature and produced through differences in emotional and motivational traits.

Experiments on the Inheritance of Behavior Patterns in the Dog

Experimental Methods

With most of the above information at hand a large general experiment on heredity and behavior in the dog was set up at the Jackson Laboratory beginning in 1945. Breeds in this species have been selected over a period of centuries for differences in behavior, but very little scientific genetic work has been done because of the expense of rearing the animals. Humphrey and Warner (29) reported success in a selection program in German Shepherd dogs for performance as guide dogs to the blind, but the data given was insufficient for genetic analysis. James (30) found, as had Pavlov before him, that temperamental differences affected the performance of dogs in a conditioned reflex situation and that there appeared to be some breed differences of this sort.

With the advice and help of a number of leading scientists, the new experiment was set up to meet the following conditions: (1) The environment is kept as uniform as possible in order to magnify genetic differences between several dog breeds which were sampled from a wide variety of behavioral types. (2) An attempt was made to set up a favorable social environment and due emphasis was placed on the study of differences in patterns of social behavior. (3) The animals are tested in a large variety of situations with tests designed to bring out differences in both patterns of behavior and learning processes. Approximately a dozen tests of each sort are given to the animals. (4) The testing is carried on throughout the first year of life of the animal, since behavior in the same individual may change markedly with later development. It is also possible to study the animal's behavior at times when it has been modified by a relatively small amount of learning and a relatively large amount. (5) Two of the most different breeds are being studied by a crossbreeding program

including reciprocal crosses, backcrosses to the same mothers, and F_2's. This and cross fostering makes it possible to study the effects of cultural as well as biological heredity.

Results*

While it is not possible to give the final results at the present time, several facts have been determined and can be reported. The work of Scott on genetic differences in patterns of behavior, and in particular social behavior, leads to the following conclusions. A comparative study of behavior patterns in domestic dogs and the wolf shows that there has been no basic change as a result of domestication and selection (31). Behavior patterns of the wolf have been suppressed or exaggerated by selection, but nothing essentially new has been added. It has been easy to find differences in patterns of behavior between the breed samples (32). For example, when puppies are trained to stand quietly on the weighing scales for a period of one minute, more than 80 per cent of Cocker Spaniels will remain quiet by 16 weeks of age. At the opposite extreme are Wire-haired Fox Terriers which show an increasing proportion of animals which are continuously active and usually playfully aggressive as they grow older. The differences are statistically highly significant.

In most of the breeds tested, the majority of the animals show timidity reactions toward human beings in early puppyhood which rapidly disappear as they grow older. Such reactions are unusually prominent in the strain of African Basenjis tested. By contrast, Cocker Spaniels show only a small amount of timidity reactions which on the average do not decrease with additional human contact. This may be considered as a genetic difference in the process of socialization and as such has many implications for human clinical studies of the effect of early environment (3).

Other examples which may be cited are the fact that Beagles show greater amounts of social investigation as puppies, that Fox Terriers show more aggressiveness than other breeds and that the African Basenjis show a lower amount of social attraction to people. A preliminary analysis of the number of genetic factors involved in the difference in attraction between Basenjis and Cocker Spaniels indicates that only a small number may be involved, agreeing with Dawson's work, although this conclusion may be modified with additional data and when additional tests are considered.

Large differences are also shown in physiological measurements which may be associated with emotional reactions. Fuller has found definite breed differences in the average heart rate, the degree to which heart rate is modified by environmental stimulation, and even in the average body temperatures (33). Such differences appear to be more definite and show less overlap than those involving patterns of behavior.

Royce (34) made a factorial analysis of 32 variables which were derived from tests of social, emotional and physiological reactions. He was able to find six well-defined factors, and it was indicated that further unidentified factors were present. Factors tentatively identified were behavioral timidity, psychophysiological timidity, hyperresponse of the heart to social stimulation, aggressiveness, activity level and audiogenic reactivity. It may be concluded that there are several identifiable traits of this type in the dog. While no factorial analysis has yet been made showing the relationship between these factors and tests of adaptability, the data indicate that emotional and physiological reactions have a profound effect on the capacity to learn and adapt in certain situations.

General adaptability. It has also been possible to study the general capacity for adaptability in the different breeds as studied under a standard environment.

*One caution needs to be made in interpreting the results. Only small samples were taken of each breed, and while the differences observed demonstrate the range of variability in the dog they are not necessarily characteristic of the entire breeds concerned.

This environment actually consists of many specific situations, and Fuller (35) has shown that, while some breeds adjust very well to most of these situations, other breeds and individuals may be limited in their capacities. For example, the Wire-haired Fox Terriers have been selected, like other Terrier breeds, for their ability to fight. They show quick emotional responses and show little fear in most situations, and this may be helpful in some kinds of learning. On the other hand, the animals are very poorly adapted to living in groups of their own kind. The normal Jackson Laboratory environment duplicates a natural situation in that the animals are raised within their own litters. Fox Terriers begin to be aggressive toward each other at 7 or 8 weeks of age and show an excessive tendency to attack one individual. These attacks become so severe that their object has to be removed in order to save its life, and we have been able only once to raise more than two or three animals together. The Fox Terriers are therefore very poorly adapted for living with other Fox Terriers.

A similar and perhaps more interesting result has been obtained with Shetland Sheepdogs. When these animals are raised in close association with human beings from early puppyhood, they become capable of learning to carry out a very highly complicated set of commands in connection with sheep-herding and are very easily trained to the proper behavior for a house dog. When raised under the Jackson Laboratory conditions, which appear to be highly satisfactory for most breeds, but in which the contact with human beings is more limited, the animals adjust satisfactorily to their littermates but perform very poorly in many tests and training situations. The same characteristic appears to account for the failure in one environment and success in another, namely, a high degree of emotional sensitivity. This trait has been measured by Fuller as heart rate change and tends to be twice as great as that seen in breeds like the Basenji and Cocker Spaniel. If such a nervous individual is given a great deal of security and is allowed to develop a close relationship with a human being, he becomes observant, quick and retentive in learning situations. Under other situations the animal may become highly timid and easily distracted so that it is quite difficult to teach. In both this case and that of the Fox Terrier there appears to be a trait or group of traits which make the animal highly adaptable in certain special situations, but which may limit adaptability in others. There is no indication that animals may possess a single trait of general adaptability.

Before going ahead to report on evidence on the ability of the dog to meet new situations, and which is more properly related to learning and intelligence, the available background information may be summarized. It has been easy to find differences between samples from the dog breeds in patterns of social behavior and the emotional and physiological reactions associated with them. Simple physiological measurements give the most clearcut differences. Certain emotional traits may make adjustment difficult in some situations, although they may facilitate learning of the type for which a breed has been selected.

Testing Genetic Differences in Learning Ability

Learning in the dog has been tested in a variety of problems. Some of these involve overt and emotional patterns of behavior, while in others an attempt has been made to eliminate these factors as far as possible. The results will be illustrated by a series of three tests which are regularly given to all animals.

A description of test procedures has been published (36), and detailed accounts of the genetic results on each type of problem are in preparation. Evidence for hereditary differences in capacity to perform in a variety of situations is found in each of the three experiments. Our experience is that

almost any psychological or physiological test is affected by the genetic background of the subjects. The genetic contribution to variance is probably more striking in physiological and emotional reactions than in pure learning situations. The first test to be described, lead training, involves emotional training rather than high-level problem solving. The other two test series, discrimination-delayed response and spatial orientation, are simple problems in which an attempt is made to accustom each animal to the test environment before actually evaluating his performance.

Leash Control Training

Leash control training covers a ten-day period, five days during the 19th week of age and 5 days at 21 weeks. Dogs are individually trained to walk over a course which includes outside paths, entrance into a building, passing through doors and climbing stairs. The objective is to train each subject to run close to the left side of the trainer on a slack lead. Experienced trainers assign demerits to each dog in five categories: (1) Balking in the open or at doors; (2) Fighting the leash; (3) Position with regard to trainer; (4) Interference with trainer, and (5) Vocalization. A perfect performance would be rated as zero; the maximum number of demerits under the scoring system is 18. Trainers alternate in handling each dog so as to reduce possible observer bias. The correlation between odd-even trials, corrected for the regression of scores on days, is 0.856, indicating that this is a reliable test of performance.

TABLE 1

COMPARISON OF BREED SAMPLES ON LEAD TRAINING

Breed	*Initial Demerits*	*Daily Change*
Basenji	9.2±.3	−1.1±.1
Beagle	8.8±.4	−1.8±.2
Cocker Spaniel	5.5±.4	− .5±.2
Shetland Sheepdog	10.3±.8	−1.4±.3
Wirehaired Terriers	5.2±.3	− .7±.1
Bas. x C.S.	9.3±.4	−1.6±.1
All dogs	8.1±.4	−1.2±.2

The results for the first five days of training are summarized in Table 1. The data for this table were obtained from 142 dogs, divided exactly equally between the sexes. In general, animals improve steadily with training, although individuals do not show the beautiful regularity of the calculated curves. The average dog of our sample obtained 8.1 demerits on the initial day of training, and improved by 1.2 points on each day of training. No difference between males and females was found on total scores, but the sexes differed significantly in the kind of demerits which they acquired. Males were more vocal, and females tended to drag behind to a greater extent.

The breed group scores were subjected to analysis of variance and the individual means were compared by Tukey's (37) technique. We may conclude that the breeds fall into three distinct groups: a high group including Basenjis and Shetland Sheepdogs; a middle group consisting of Beagles and Basenji × Cocker hybrids; and a low group of Cocker Spaniels and Wirehaired Terriers. A more sensitive type of comparison involves the five categories of demerits which were recorded separately. Figure 1 shows the results. (See p. 43.)

From left to right the columns for each breed represent total demerits in five categories, fighting the lead, position, vocalization, balking and interference. Not only do the breeds differ widely in total score, but the part-score profiles are highly characteristic. We have employed the relationship test of Osgood and Suci (38) to determine simultaneously the degree of profile and absolute score resemblance between breeds. By this method the Beagles, Cocker Spaniels and Basenji × Cocker Spaniel hybrids form a relatively close group; the other three groups are widely separated, both from this median group and from each other. The individuation of response patterns appears to be strongly influenced by heredity. The possible significance of this approach to the problem of how hered-

ity affects intelligent behavior will be taken up later.

Discrimination and Delayed Response

Lead training involves learning to inhibit non-adaptive responses and may be considered simpler than learning to solve problems. The discrimination-delayed response series of tests requires that the subjects learn to associate a swinging panel with an open corridor leading to food. A stationary panel indicates a closed corridor. The apparatus is T-shaped and contains a movable barrier so that subjects may be forced into one or another of the arms of the T as a part of the training process. Animals "pass" the test when they have a run of correct responses significant at the 1 per cent level (39), or when there is a similarly significant preponderance of correct responses over specified blocks of the testing according to the Chi-square test. A dog is considered to fail if he does not meet either criterion within 158 training trials. Dogs which pass are given 50 additional trials and are then tested on delayed response. For this test the procedure is altered by dropping a screen over the starting box, thus cutting off the view of the moving panel. The dog is released after various periods and must base his choice upon some sort of memory of the stimulus (or of his anticipated response).

Three different comparisons of results on the cue response test are presented in Table 2. These are the per cent of subjects meeting the criterion, the average number of trials needed by the successful subjects to meet the criterion, and the percentage of correct responses in the 50-trial test period. The order of ranking of the breed samples varies according to the measure we select. Analysis of variance has been used to evaluate the probability that the breed samples are really different in rate of training and in consistency of response after being trained. Again using Tukey's method for comparing individual means, the samples of Beagles and Wirehaired Terriers appear as superior performers. The results with the Basenjis, Cocker Spaniels and their hybrids show great overlap between genetic groups. Probably another hybridization experiment will yield more information on the hereditary factors in this particular performance than will the Cocker-Basenji cross which has been productive in other phases of the project.

Passing on to a consideration of breed differences in the delayed response test, we must restrict our study to those subjects which have previously met the criterion for discrimination. This reduces the sample size and introduces a possible selective factor. Hence these results have not been subjected to detailed genetic analysis. However, the sample is large enough for us to make certain deductions regarding the relationship between the ability to respond correctly in a discrimination test and the ability to respond in the same situation when the delay factor is introduced. Table 3 summarizes the relationship between the level of delay attained and the previous performance on the cue response test.

The dogs which failed to establish a satisfactory delayed response performance dur-

TABLE 2

A Comparison of Performance of Breeds of Dogs on the Cue Response Test

Breed or hybrid	N	*Passing*	*Rank*	*Trials to criterion** Average	Rank	*Post-Criterion** Percent Correct	Rank
Basenji	17	12	3	68	5	74	5
Beagle	15	13	2	24	1	88	1
C. Spaniel	15	10	5	53	4	77	3
W.H. Terrier	15	13	1	39	3	84	2
Bas. x C.S.	18	11	6	37	2	71	6
C.S. x Bas.	22	15	4	87	6	76	4

*Values in these columns are based upon animals meeting the pass criterion.

TABLE 3

RELATIONSHIP BETWEEN ACCURACY OF CUE RESPONSE AND ABILITY TO PERFORM DELAYED RESPONSE

Level of delay attained. Sec.	0	1	5	10+
Number	33	19	13	17
Mean percent correct—C.R.	73	84	77	86
Range of percent correct—C.R.	50-94	56-100	56-92	66-100

ing their training period had the lowest average performance on the cue response test, while the dogs with best performance on delay were highest on cue response. However, there is no clear trend in the figures for dogs with intermediate performance, and the range of response and performance within categories of delayed response performance is very large. We conclude that the two tests are related, but that they measure somewhat different abilities.

At the present time the data is insufficient to make definite conclusions regarding differences in the ability to delay response in our breed samples. It is probable that Beagles and Wirehaired Terriers are significantly better than the other animals, but we will need larger numbers before making a definitive statement. The abilities involved in the delayed response test have been widely studied by psychologists. Most of the work has been done with primates. A high level performance seems to depend upon the integrity of the cerebral frontal lobes (40). However, even following destruction of these areas it has been found possible to improve performance by means of sedative drugs or by reducing the environmental stimuli so that no distracting factors are present during the delay period (41, 42). We have shown with dogs that success in the delayed response is closely correlated with the ease of eliciting the orientation reaction to the stimulus. Should the apparent genetic differences in the delayed response ability be confirmed with larger numbers of subjects, there is an opportunity to relate this difference to brain areas which are known to be of particular importance for it. It must be emphasized that the correlation of delayed response performance, with a genetic difference on one hand and a neurological deficit on the other, does not lead to the conclusion that heredity operates by producing a neurological defect. This is merely a lead which must be investigated by direct methods.

Spatial Orientation

The final test to be considered has been called spatial orientation. This is related to the multiple choice test which has been previously used to study the intelligence level of animals. The apparatus consists of an elevated structure, accessible by ramps, which is placed in the animals' own living quarters so that they have ample opportunity to explore it. There are three alternative pathways to the goal table. By rotating a cage on top of the table, all but one approach can be blocked off. The same approach is never correct on two successive trials. The problem differs from a classical multiple choice test in having the three approaches quite distinct and in allowing for the possibility of a completely visual solution. Errors in this test consist of climbing the wrong ramp, attempting to get on the apparatus at points other than the ramps, and any deviation from a path which leads directly from one ramp to another. As in the other tests described, there are differences between the genetic stocks in their ability on this test. Figure 2 (p. 43), summarizes the results for six consecutive trials on the first problem. Initial differences in performance persist throughout the series. On a second similar problem the Basenjis and Wirehaired Terriers continued to make more errors. The correlation between error scores on the two prob-

lems is .60. This is indicative of a highly significant relationship between the two problems, but it is not high enough for a very accurate prediction of an individual's performance. The breed differences are significant on each trial except the last.

Table 4 summarizes some results of a correlational analysis of the spatial orientation test. The upper row of the table gives the intraclass correlation which is the ratio between the variance attributable to genetic effects operating within the breed samples to the variance of individuals picked at random from the whole population (43). The higher values indicate greater efficiency in bringing out genetic differences. The second row gives the correlation between adjacent pairs of trials for individual scores. There is no distinct trend in the value of the intraclass correlation coefficient. In other terms the test does not increase nor decrease systematically in efficiency as a means of detecting hereditary differences. However, the efficiency of the test as a predictor of individual performance rises with repetition to a maximum of approximately 0.80. The interpretation of this paradox will require further research into the generality of the phenomenon.

TABLE 4

INTRACLASS CORRELATION (R_I) AND BETWEEN TRIAL TETRACHORIC CORRELATION (R_{Tet}) FOR SPATIAL ORIENTATION TEST

Trial	1	2	3	4	5	6
R_I	.16	.18	.07	.19	.21	.06
R_{Tet}	.30	.51	.74	.79	.78	

Correlation between Tests

Perhaps the most interesting question which may be asked at this point is whether the ability to do one test well is correlated with the ability on other tests. The three tests which have been described are quite dissimilar, and one might predict that there would be no relationship between them; on the other hand, one might suspect that some general factor, such as docility, timidity, or specific relationship to the experimenter, might pervade all the tests.

The scores for all three tests were converted to a standard 10-point scale on which 1 represents the poorest 2 per cent of the cases and 10 the best 2 per cent of the cases. Subjects are assigned intermediate scores so as to result in a normal distribution of scores in the population. This population will have a mean score of 5.5 with a standard deviation of 2. Justification for forcing scores arbitrarily into a normal distribution is based upon the fact that measurements of behavior do not have a firm physical substrate such as do weights and lengths. The difference between 100 errors and 150 errors is not the same as the difference between 5 errors and 55 errors. Most measurements of behavior are the resultant of numerous forces acting in various directions. In such cases one would expect the behavioral characteristics to be distributed into approximately normal distribution. Hence, when there is apparent discontinuity in the measurement of a behavior pattern the measurement scales should be carefully scrutinized. Transformation to a normal form of distribution will ordinarily facilitate analysis of results. Of course, either experimental and sampling procedures, genetic deviations or threshold effects may interfere with normality of distribution of the measured trait. In our experiments we have attempted to keep the environmental factors as consistent as possible for all animals and a standardization of scores has been based as far as practicable upon a population sample drawn from a diversity of genetic types. Tetrachoric correlation coefficients between these three tests were calculated by the method of Davidoff and Goheen (44). The values are: lead training and discrimination, 0.37; lead training and spatial orientation, −.12; discrimination and spatial orientation, −.17. The low degree of association between scores indicates that no single factor of intelligence or docility can account for their distributions.

The specificity of learning ability in these three situations gives some clues to the kind of factors which may be operating. Hall (45) states that many studies have shown generality in maze learning ability, fearfulness, aggressiveness, and exploratory drive. Hunger and thirst drives, however, appear to be situation specific. Breed differences are more clearcut in the leash control test which includes many emotional responses, but they are also found in the two tests where an effort was made to eliminate emotional disturbance. Observation during testing suggests that residual emotional disturbance and low food motivation may account for some poor individual performances. When the genetic experiment is completed and the learning and emotional test results compared in detail, we may know more about the relation of hereditary differences in emotionality and drive specificity to learning in different situations.

How Does Heredity Operate?

What sort of general conclusions can be drawn from this research? In the first place, it appears that hereditary differences are shown more clearly as one analyzes test results in more detail. For example, the differences in leash control might be attributed to variation along a single scale of docility. An alternate view would be that genetics works by increasing or decreasing the probability of particular motor responses under particular stimulus conditions. These responses are the fundamental genetic traits, and their grouping into such categories as docility, timidity, intelligence, and the like are artificial constructs which are useful in certain experimental situations. It is not logically necessary that the genetic organization of behavior correspond with the organization which psychological science has constantly employed. It is possible that the analysis of the way in which heredity contributes to individual differences in behavior can assist in the isolation of behavioral categories which are particularly amenable to physiological and genetic interpretation.

Whenever the student of behavior has looked for genetic differences in capacity or type of response, he has found them. It is likewise true that the physiologist who compares different species or different strains within a species finds that the genes have an influence upon diverse organic processes of the body. Since genetic effects can be demonstrated both on behavior and on physiological processes, it might be expected that correlations between events on the psychological and physiological levels within genetic strains would yield valuable information relating the two levels of integration. Attempts to make such correlations in humans have not met critical standards. In animal subjects spurious correlations may result from the chance isolation of two unrelated processes within the same line. However, the method appears to have usefulness, in spite of obvious difficulties. Of course, the physical correlates of intelligence and personality may be tucked away so deeply within the cerebral cortex that they will evade our most persistent search. It must be admitted that except for Lashley (46), practically no one has looked very hard in the nervous system for individual differences of organic pattern. It has been easier to study more accessible systems even though their relationship to behavior must be less intimate.

There is also a strong possibility that the structure of behavior is molded to the contour of a very large number of organic foundation blocks and bears no simple relationship to any organic characteristic considered in isolation. The use of genetic techniques may aid in testing this possibility, since individuals can be synthesized who will vary independently in each separate characteristic. The experiment which has been described in part in this paper represents a modest beginning.

Summary

Genetics may conceivably affect adaptive

behavior either by providing the animal with fixed methods of adaptation for often repeated environmental situations, or it may affect the ability of the animal to learn to adapt to novel situations, or both. The evidence from studies of species differences in behavior generally confirms the theory that genetic differences in patterns of behavior exist, although in the species of mammals which have been thoroughly studied, there is also evidence of a cultural inheritance of behavior patterns. In any case, genetics does not put adaptive behavior in a strait-jacket, since in any species there is a variety of behavioral patterns which are tried out when adjustment is not immediately obtained.

Studies on differences between species in capabilities for learning and meeting new situations are not so satisfactory. There is evidence that the basic processes of learning are very similar throughout the vertebrates and probably the entire animal kingdom. There is also evidence for existence of complex learning abilities, but whether these exist apart from the differences in sensory and motor capacities and the limitations imposed by special patterns of behavior is still a matter of some doubt.

Within species there is again abundant evidence of genetic differences in patterns of behavior involving both voluntary and involuntary patterns of response. There is also evidence that the ability to learn in specific situations is strongly modified by differences in patterns of behavior, particularly those involving emotional reactions. So far, there has been no clear evidence of genetic differences in learning capacities which exist apart from differences in behavior patterns and their emotional and motivational causes, or which are associated with gross neurological defects.

Both between species and within species there is evidence of genetic differences in the ability to adapt to a wide variety of environmental situations. When such cases are analyzed, it is found that these are the result of special patterns of response which tend to make the animal highly adaptable in certain situations but may limit its power of adjustment in other situations.

Genetic differences have been measured in the dog in a wide variety of situations which emphasize learning as well as those which bring out differences in patterns of behavior and motivational and emotional reactions. Certain general findings have been made. There are a variety of social and emotional traits which may affect learning, and which are related to the learning tasks for which the breeds have been selected. Genetic differences have also been demonstrated in a variety of learning situations. In any kind of test, part scores give more clearcut genetic differences than measures of overall adjustment. Likewise, there may be very little correlation between learning in one situation and another. All this is consistent with results on other animals and points to the *specificity* of genetic effects on behavior. The physiological and structural basis of this specificity is yet to be discovered.

A Program for the Future

Animal research may be expected to make significant contributions in the area of psychogenetics, since genetic techniques can be utilized at full efficiency only with laboratory animals. These studies can be continuously integrated with those performed on humans. The situation is something like that of relating brain function to behavior. Planned experiments are possible only with the lower animals, but accidental injuries to man allow us to make valuable comparisons. Nature performs genetic experiments with man also, in producing identical twins and in perpetuating mutations which are linked with specific metabolic errors. A program of research for the future must include:

1. Continued study of the heredity of specific simple elements of behavior. This is best done with pure lines of animals, but studies on twins can also contribute.

2. Further research on the organization of intelligence in both man and his mammalian relatives. We must know more about what kinds of differences to look for between genetic groups.

3. Continued search for structural substrates of behavior which are gene controlled. These may exist at the molecular level where they will be studied by the chemist, or at grosser levels where the biologist will be in charge.

4. Continued interest from psychologists, geneticists, physiologists, psychiatrists, educators and others interested in human welfare. There are signs that the cold war between environmentalists and hereditarians is grinding to a halt, because neither side can support an extreme position. The time for a comprehensive theory of the relationship between heredity and intelligence is not here, but we are in the best position ever to collect new facts, evaluate them in the light of other facts, develop limited theories and put them to experimental test.

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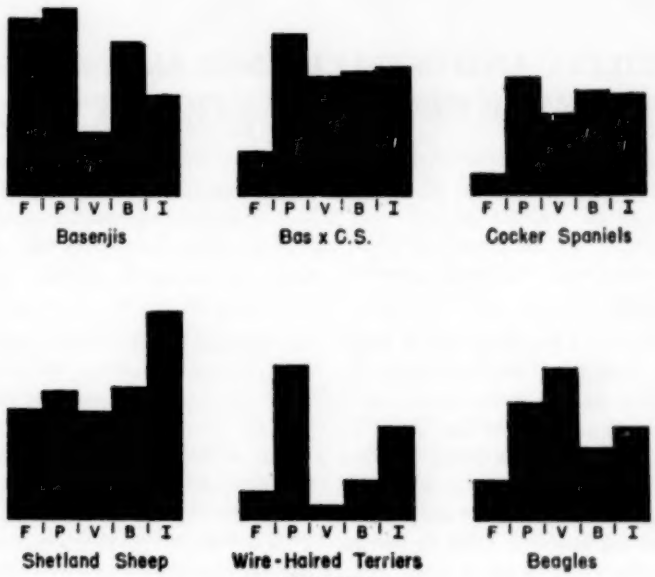

FIGURE 1

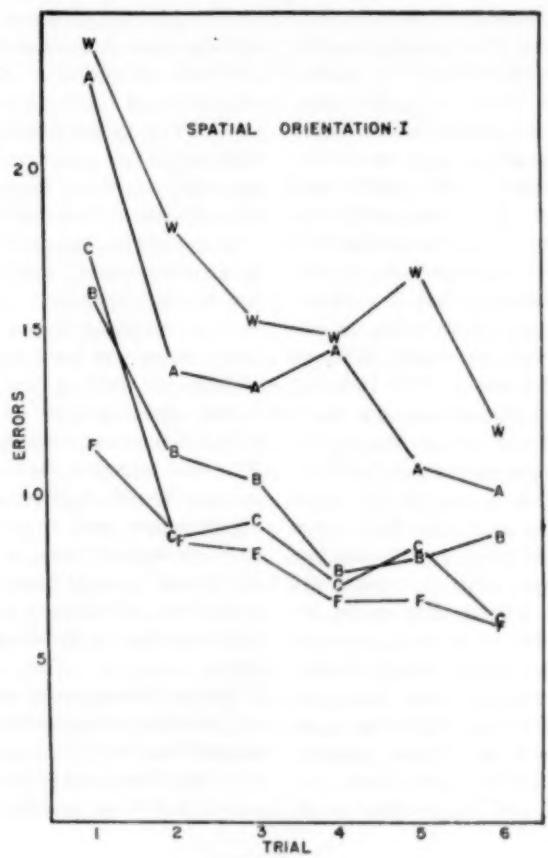

FIGURE 2

II. FERTILITY AND INTELLIGENCE AMONG FAMILIES OF THE MENTALLY DEFICIENT

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One of the large projects at the Dight Institute is a study of mental deficiency. This would seem to have considerable social significance because the mentally deficient are not normal at birth. By definition they do not function in a socially adequate way and they serve mainly to test the quality of our charity. They do not get well, nor do they usually die young.

Great progress has been made in curing the bodies of the mentally deficient, exclusive of their brains. No striking progress has been made in discovering ways of overruling the hosts of different genes responsible for the various diseases known as mental deficiency. Recently considerable attention has been devoted to the valiant hope that glutamic acid might rehabilitate the mentally deficient. It is conceivable that glutamic acid might improve a few specific types of mental deficiency, but it is unlikely that it would have much effect on any large proportion of the many different kinds of mental deficiency. The discovery of a "cure" for the phenylketonuric imbecile, if possible, would not be expected to be also a cure for the microcephalic idiot.

It is obvious to the geneticist that many of the various kinds of mental deficiencies result from different genes at different loci on the chromosomes and that until the different kinds of mental deficiencies are named and described not much medical progress toward cures can be expected. One important way of learning more about the different types of mental deficiency is to study the families of the affected persons. This is not a particularly novel idea, but no large, adequate, and longitudinal study of the families of the mentally deficient has been produced so far. Our study is large and longitudinal but also inadequate in several ways. However, it does provide some description of the families of the mentally deficient, particularly in regard to their intelligence and fertility.

It is well known that if one takes a group of school children and arranges the children according to their I. Q.'s, those with the highest I. Q.'s come from the smallest sibships and those with lower I. Q.'s from the larger families. This negative correlation between the intelligence quotient and size of family has been known for a long time and has given rise to considerable apprehension that because of the differential fertility of those with different intellectual capacities, the genes for high intelligence are gradually being replaced by those for low intelligence.

It is surprising that no one has made a study as to the later reproduction of the sibships of varying size and intelligence. There are a number of reasons why this highly important work has not been done. The first reason is that one ought to wait for the tested children to complete their reproduction and then find them again and test their children, or else take a group of people already past the reproduction period and search back to find I. Q. values obtained for them when they were children.

We are fortunate in our project in having family genealogies which were carefully worked out forty years ago with some I.Q.'s obtained then, and it has been possible to search out large numbers of I. Q.'s since

then. We have taken advantage of both of the above methods. When our study is completed there should be adequate material to demonstrate the reproductive capacities of people with I. Q.'s up to perhaps 120; the sample of persons with I. Q. values above 120 will probably be too small to give valid answers. However, the reproductive characteristics of people with I.Q.'s above 135 are known from Professor Terman's longitudinal study of gifted children, while the gap between I. Q. values of 120 and 135 perhaps can be filled satisfactorily by interpolation. At this moment we will be content to consider only the sibships of the mentally deficient who were institutionalized at the State School and Colony at Faribault, Minnesota, forty or more years ago.

Our project is a follow-up study of some of the patients who were in the Institution for the mentally deficient at Faribault, Minnesota, some forty years ago. The original work took place between the years of 1911 and 1918 and was partly supported by an appropriation of $25,000 from the Legislature of the State of Minnesota. The late Dr. A. C. Rogers, then Superintendent of the State School and Colony at Faribault, was responsible in large part for the grant by the Legislature. The grant was used for the research and travel expenses of the workers on the project, while their salaries were provided by the Eugenics Records Office of the Carnegie Institution at Cold Spring Harbor, New York. There were two investigators employed, both college graduates, with training in social work and in what was then known of human heredity. They were the late Miss Sadie Deavitt and Miss Marie Curial, now the wife of the Reverend Guy Menefee of Rochester, Minnesota. We are greatly indebted to Mrs. Menefee for information concerning the original work. The early Stanford-Binet tests were carried out by Dr. Maud A. Merrill, Miss Mildred Thomson and others.

The illness and death of Dr. Rogers brought the study to an end, and it was never published. The original data in excellent condition went to the Division of Public Institutions in St. Paul and copies were sent to the Eugenics Records Office in Cold Spring Harbor. Both sets of records were presented to the Dight Institute for Human Genetics many years later.

In 1949 the study was reopened. The new research has been supported by the Minnesota Human Genetics League, by a contract number E-1126 with the Division of Public Institutions, and by the Rockefeller Foundation. It is a pleasure to point out that our work has been dependent upon the cooperation and generous assistance of Miss Mildred Thomson, head of the bureau for the mentally deficient and epileptic, State of Minnesota, and her staff. In addition we are deeply indebted to Dr. E. J. Engberg, Dr. Ralph Rossen, Dr. R. J. Gully, Dr. John Pearson, Mr. C. J. Jackson, Miss Caroline Perkins and Mr. Wendell Swenson of the Division of Public Institutions, Dr. J. A. Böök of the State Institute for Human Genetics at Uppsala, Sweden, Dr. John Schut of the State Hospital at Galesburg, Illinois, and Dr. Ray Anderson of the Dight Institute, for necessary help with the work. We are also indebted to an army of school superintendents, school psychologists and others who have provided us with I. Q. values.

It was found very soon that the records had been most carefully and conscientiously compiled by the original workers. Extensive sociological data were available for the patients, their sibs, parents, grandparents, aunts, uncles and cousins, as well as some I. Q. values and other information such as predictions as to the possible fate of various children. We have had considerable fun checking on these predictions, most of which were quite shrewd and borne out by subsequent events. However, in one case, a rascal from whom no good was expected became a United States Congressman from Minnesota!

Selection of the Sample

Our material is *not* a random sample of any population. Indeed, it is almost impossible to get a truly random sample of a human population. Our sample is biased in various ways, the most obvious bias being that in every case the propositus, or index case, was institutionalized for some part of his life, whereas about nine-tenths of the mentally deficient population of the country is never institutionalized.

The original sample of 549 propositi was selected in the following way. It was intended to complete the family histories for all of the approximately 1500 patients then at the State School and Colony in Faribault. This was too ambitious a goal, and the changing population of any large institution would make a complete sample very difficult to achieve. In order to start the project, it was decided to select first those patients whose next of kin were geographically closest to Faribault, as travel by train and horse was slow. One of the workers, Miss Curial, though now in her seventies, still shoots a deer or brace of grouse each season and at that time drove a pair of horses that were considered too fast to be handled by a woman.

No attempt was made to select patients because of their type of abnormality or I. Q. No attempt was made to select those with relatives also in the institution.

A drastic subsequent selection from the original 549 family histories has been made for the present study. We have eliminated many of the histories from our study according to definite criteria. In 33 cases the original patient had an I. Q. of 70 or more. Most of these were mentally ill rather than mentally deficient; one of them had an I. Q. of 112 and was clearly psychotic. It was found that 38 of the propositi were not actually in the institution at the time of the original study. Their family histories found their way into the group because of unusual circumstances of one kind or other. The third and largest group of histories set aside were the 182 histories of epileptic patients, some of whom were mentally deficient and others were not. We do not understand the relationship between mental deficiency and the epilepsy which often accompanies it, and therefore wish to avoid that complication for this study.

Of the original 549 families, we have set aside the above three groups totaling 253 histories, leaving 296 as the sample for our present study. Our 296 patients have already totaled 6725 years in institutions, an average of 22.7 years each. Five per cent of the patients have averaged over 50 years in Faribault and one man has been placed in a home for the aged after 65 years at the State School and Colony. Only nineteen per cent of the patients were in the Institution for less than six years. The I. Q. for all but six of the 296 patients was taken by the original workers and the average I. Q. = 38.

We have followed the lives of the propositi, of whom at least 80 (27 per cent) are still alive after forty years, which is remarkable when it is realized that many were already old at the time of the original study.

Fertility of the Propositi

We expect to find a very low fertility for the propositi because they were all institutionalized for some part of their lives. Only 37 (12.5 per cent) were known to have had any children. The 37 propositi had an average I.Q. of 48 while their 80 children had an average I.Q. of 71. The average I.Q. of the children of the mentally deficient will always be higher than the I.Q. of their parents due to Galton's law of filial regression. Half of the children were mentally deficient and half were not.

If we average the 80 children over the 296 propositi, we have an average of 0.27 children per institutionalized mentally deficient subject.

There were 21 sibs or parents of propositi who were also institutionalized and produced children. The average I.Q. = 61 for the 21 relatives is higher than that of

the propositi, but the average I.Q. = 73 of their 65 children is only slightly higher, and not significantly so, than that of the children of the propositi.

The low fertility of the institutionalized patients and relatives is to be expected and is introduced only for the record and to remind us that our expensive institutions are effective eugenic agencies and without institutionalization the total of 145 children of the above two groups of persons would have been greater. Had the propositi and relatives been committed before they reached reproductive age, the 145 children would not have been born and considerable misery and expense would have been avoided.

Scope of the Present Study

The high quality of the original research allows us to work back *to* the grandparents of the propositi, though I.Q. values could not have been obtained for them as they died before the invention of the tests. It would be impossible today to get any good information about these people, long since dead. But because of the early work we have good records for these early generations. We are at present able to follow up the children of the original study and their offspring. Consequently, we have masses of information for three generations, usually for four and sometimes five generations.

We have set an arbitrary pattern of studying *all* descendants from the four grandparents of the propositus. This gives four generations of people which are now completed but with part of the fifth generation yet to be born.

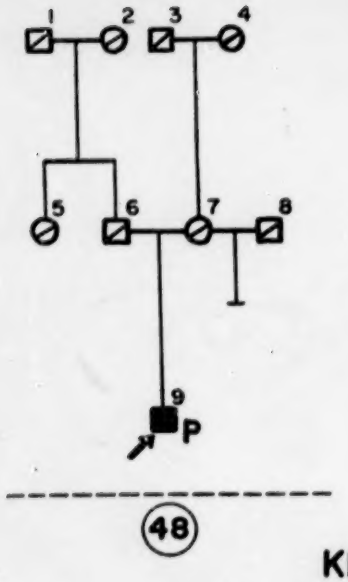
If a family of this structure were to merely replace itself, there would be the four grandparents of the first generation, six persons in the second generation, 12 in the third and 24 in the fourth, a total kindred of 46 persons. No such small family pattern would be expected on the average in a young pioneer state such as the Minnesota of the years when our propositi and their parents were born. We would expect all segments of the population to replace themselves several times over. The families of our study were large and because of that our project is rather gargantuan. We have done some work on the families of all 296 propositi, but so far have completed only 54 of the kindreds, about one-sixth of the total. The average size of these kindreds was 158 persons, which means that 8532 persons are included so far. If this is one-sixth of the total population, we should have almost 50,000 persons included in our 296 kindreds, when we finish the study.

Illustrative Kindreds

Let us examine some of the 54 kindreds individually.

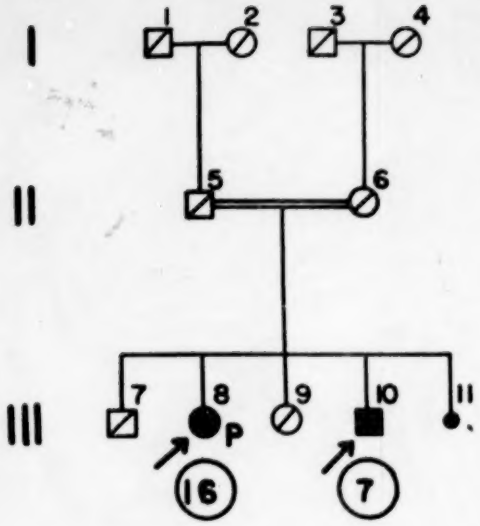
The idea that *all* the families of the mentally deficient breed like rabbits is, of course, not true, as the first illustration shows, Figure 1 (KMD 146). Here the mother of the propositus was an only child and the father had only one sister who died at 16 years of age. The patient (P) was mentally deficient without obvious physical stigmata and later became mentally disturbed, and died at 37 years of age. The boy went to school for seven years but could not get through the second grade. He was committed at 14 years of age. The home conditions seemed to be adequate, and he was treated kindly there. This family has become extinct.

Figure 2 (KMD 477) is another small family. The arrow indicates that the person was institutionalized. In this case there were two idiots, numbered 8 and 10 on the pedigree, both were in the institution at Faribault and the "P" indicates that number 8 was the proposita. The blackening of the symbols indicates the mental deficiency and the numbers encircled below the two defective children are their I.Q. values of 16 and 7. The small blackened circle shown as number 11 represents a still birth. The diagonal slash through all the male and female symbols means that all these people were assumed to be normal but that no definite evidence to that effect



KMD 146

Figure 1. The smallest family of the study. The meaning of the symbols is described in the text, each new symbol being described as it enters the more complicated pedigrees.



KMD 477

Figure 2. Another small family in which two deficient children were part of the progeny from a first cousin marriage.

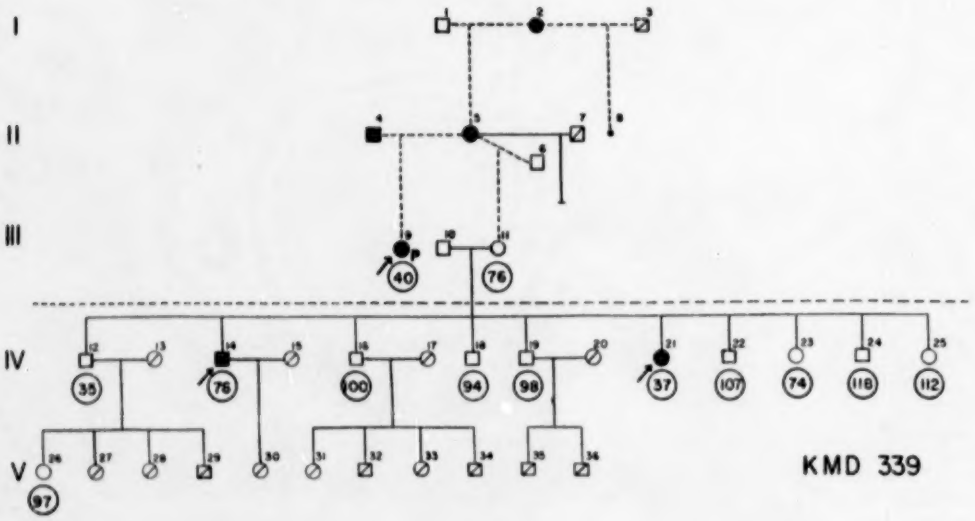


Figure 4. This family combines mental deficiency and an hereditary deafness. Three persons (12, 21 and 26) have all been resident at some time at the Minnesota State School for the Deaf. Note the wide variation in intelligence shown in the ten children of generation IV.

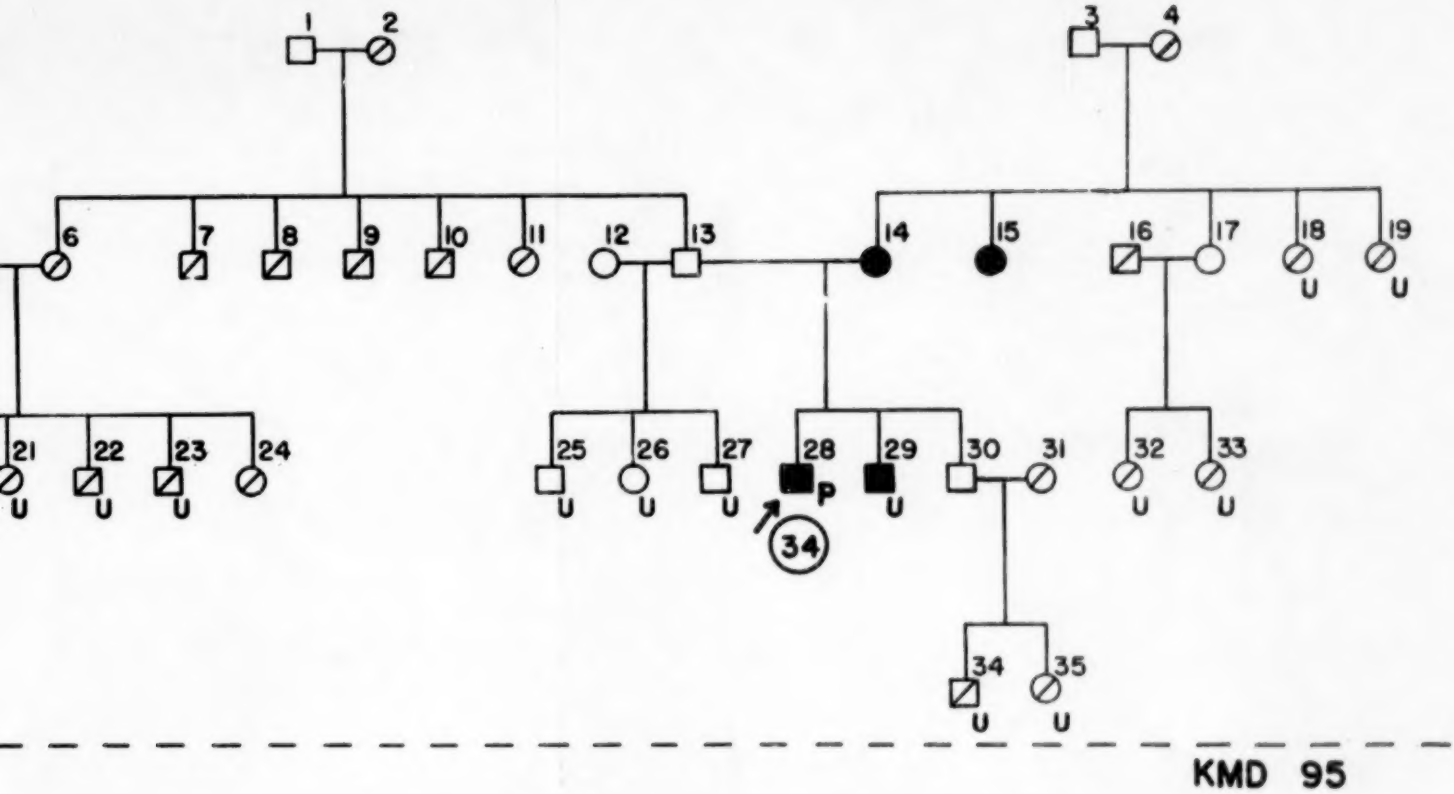


Figure 3. The greatest failure of the study. All members of the family have been lost from our study and cannot be found.

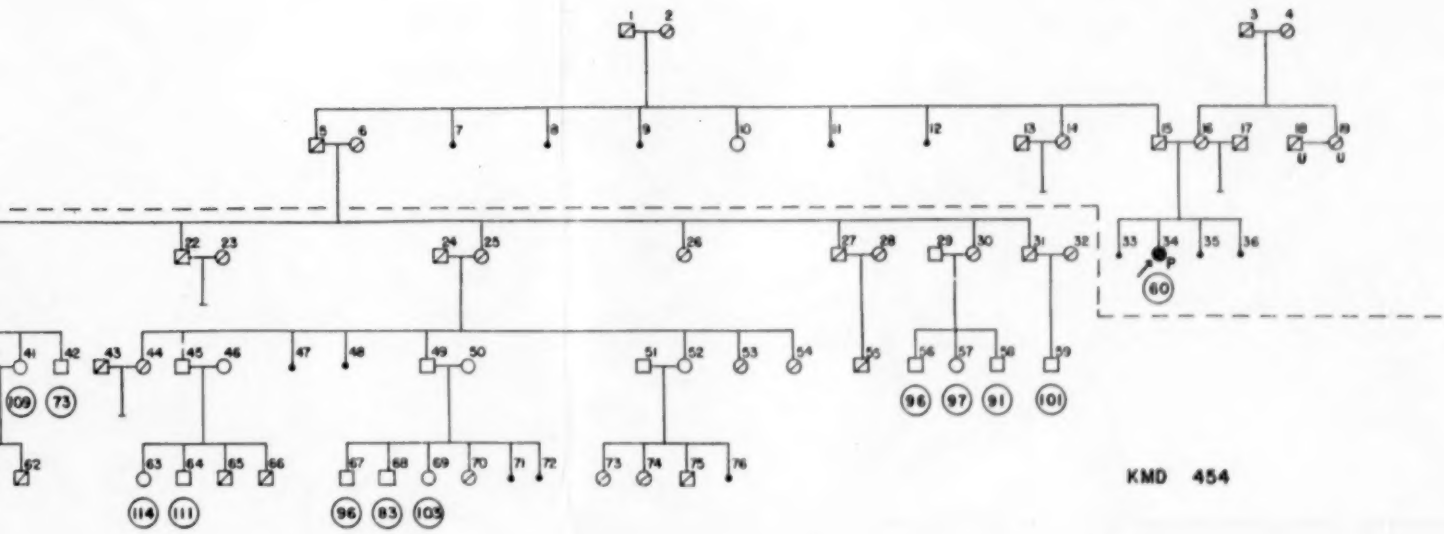


Figure 5. This family was severely limited in size by the large proportion of still births. Spina bifida and hydrocephalus were present in the sibship of the proposita at least (33 through 36).

Figure 4. This family combines mental deficiency and an hereditary deafness. Three persons (12, 21 and 26) have all been resident at some time at the Minnesota State School for the Deaf. Note the wide variation in intelligence shown in the ten children of generation IV.

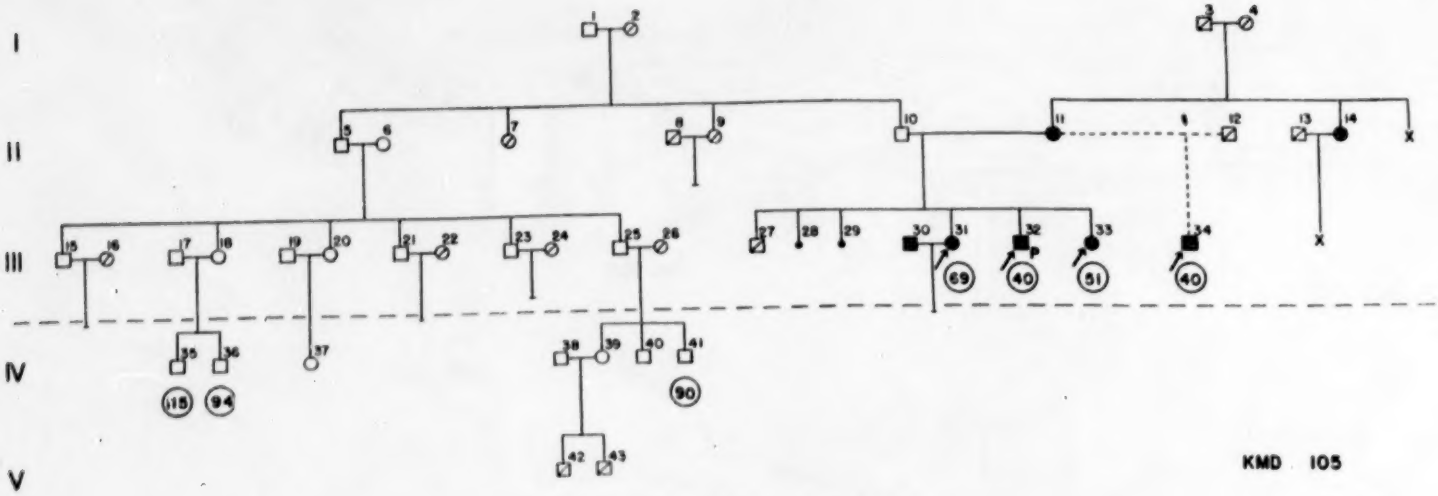


Figure 6. A family of the "Kallikak" type.

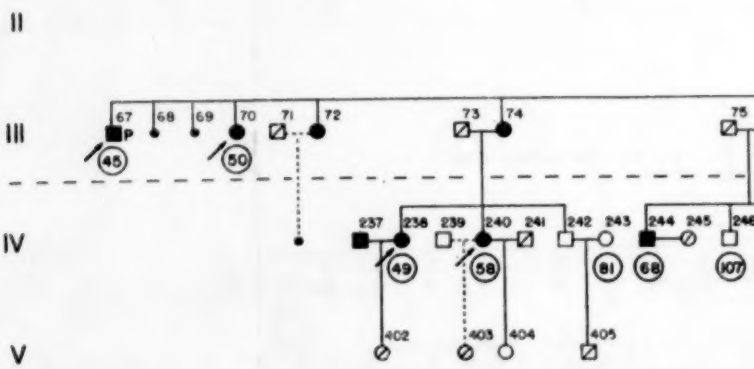


Figure 8. Part of the whole pedigree shown in Fi
four consecutive generations of I.Q. values is not c
the descendants of 17 and 18 have been to th
members of the family who have been in the Stat
given for other individuals, such as (250), who have

Figure 5. This family was severely limited in size by the large proportion of still births. Spina bifida and hydrocephalus were present in the sibship of the proposita at least (33 through 36).

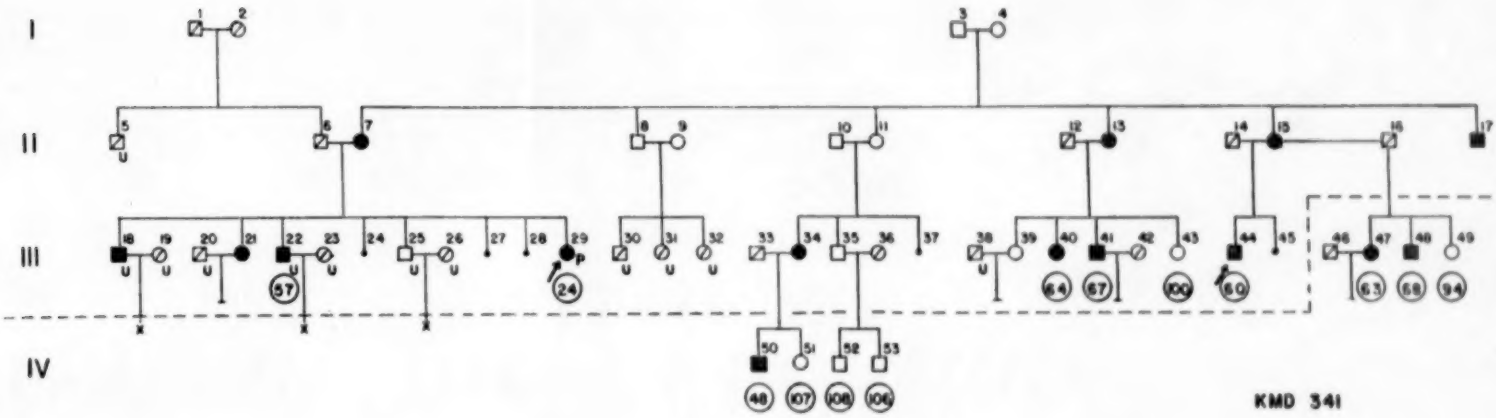
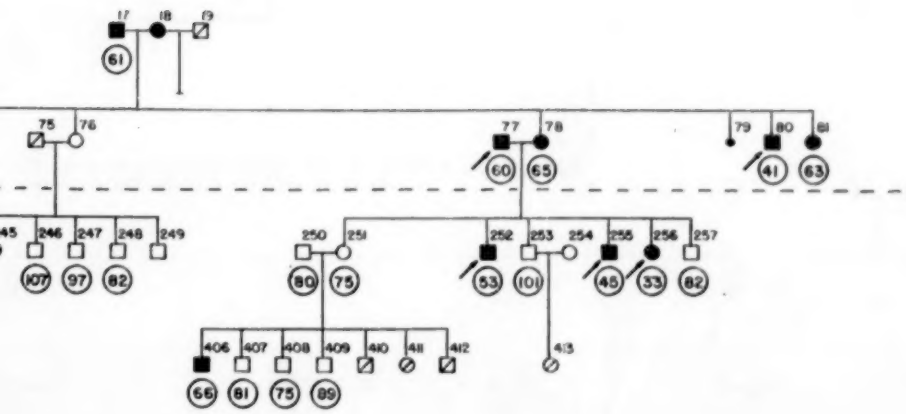


Figure 7. There is a large amount of mental deficiency in this family which suggests the inheritance of a single dominant gene causing some still undifferentiated type of mental deficiency.

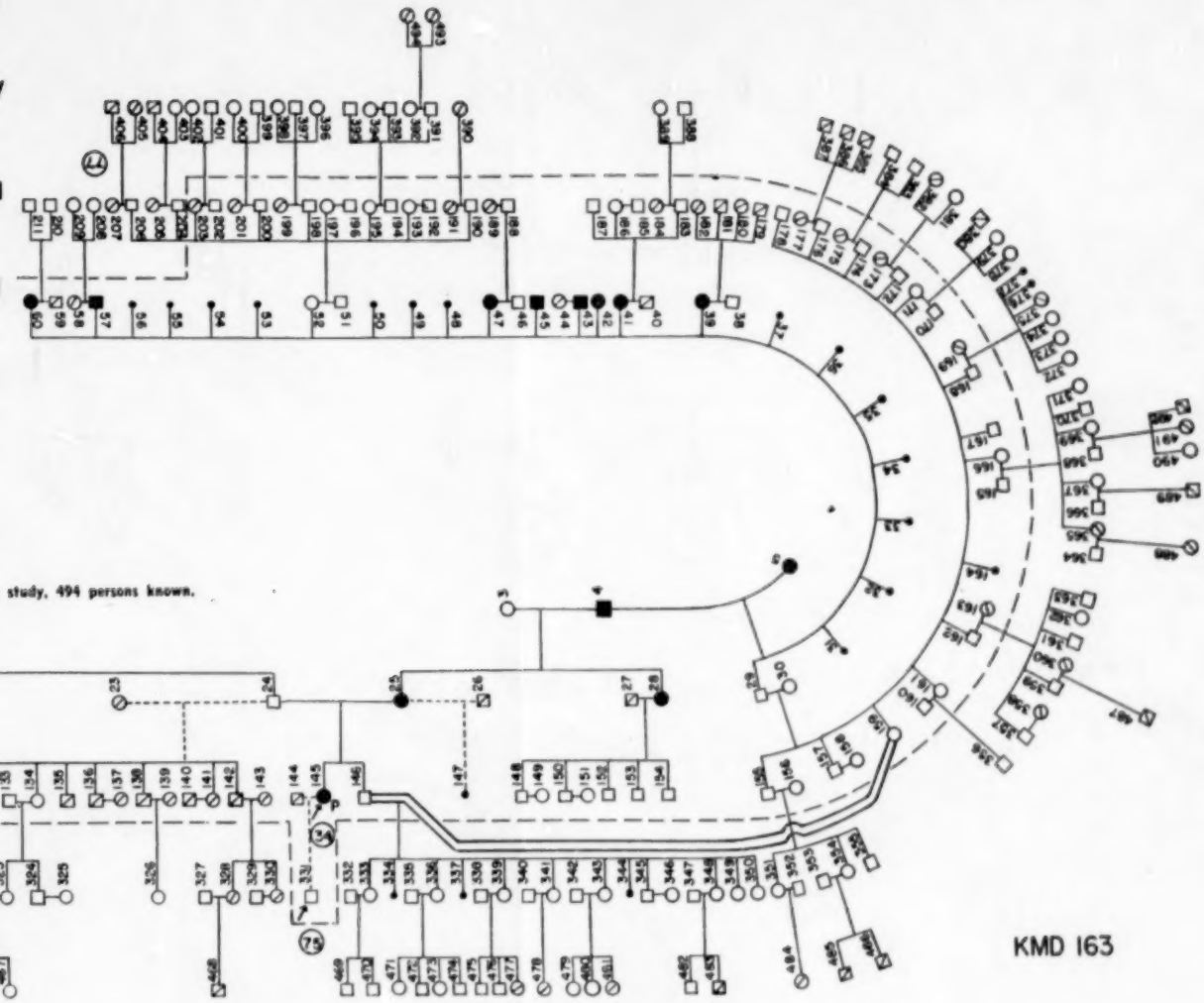


Detail from KMD 342

n in Figure 11. The fact that we are able to present
s not only a record, but is a measure of the trial which
to the State of Minnesota. The arrows indicate the
e State School and Colony at Faribault. No symbol is
o have served some time in prison or in other institutions.

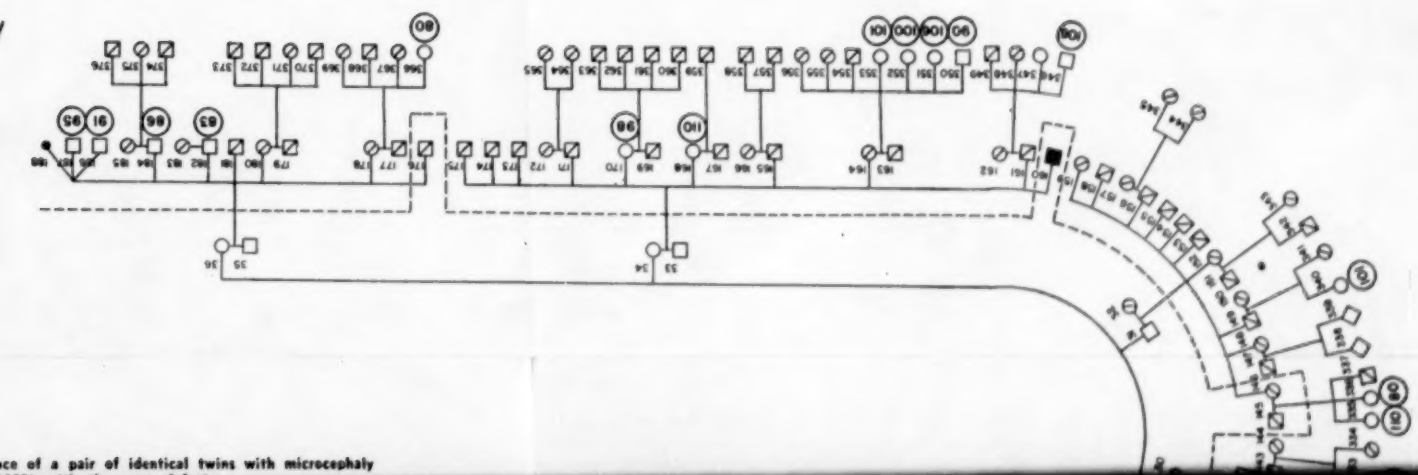
Figure 9. The largest family in the study, 4

Figure 10. This family is of interest because of the appearance of a
(111 and 112) followed by a brother (1130) wit



study, 494 persons known.

KMD 163



ce of a pair of identical twins with microcephaly
130) with the same defect

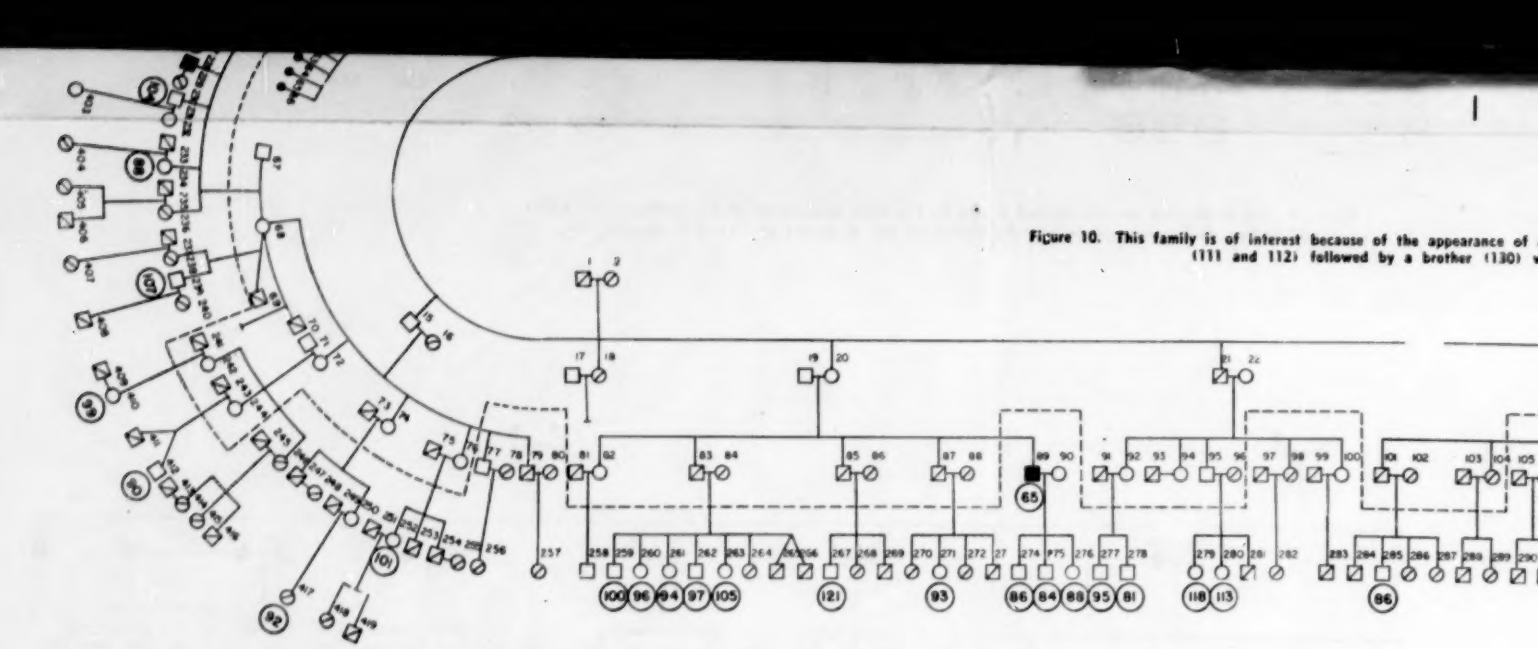


Figure 10. This family is of interest because of the appearance of
(111 and 112) followed by a brother (130)

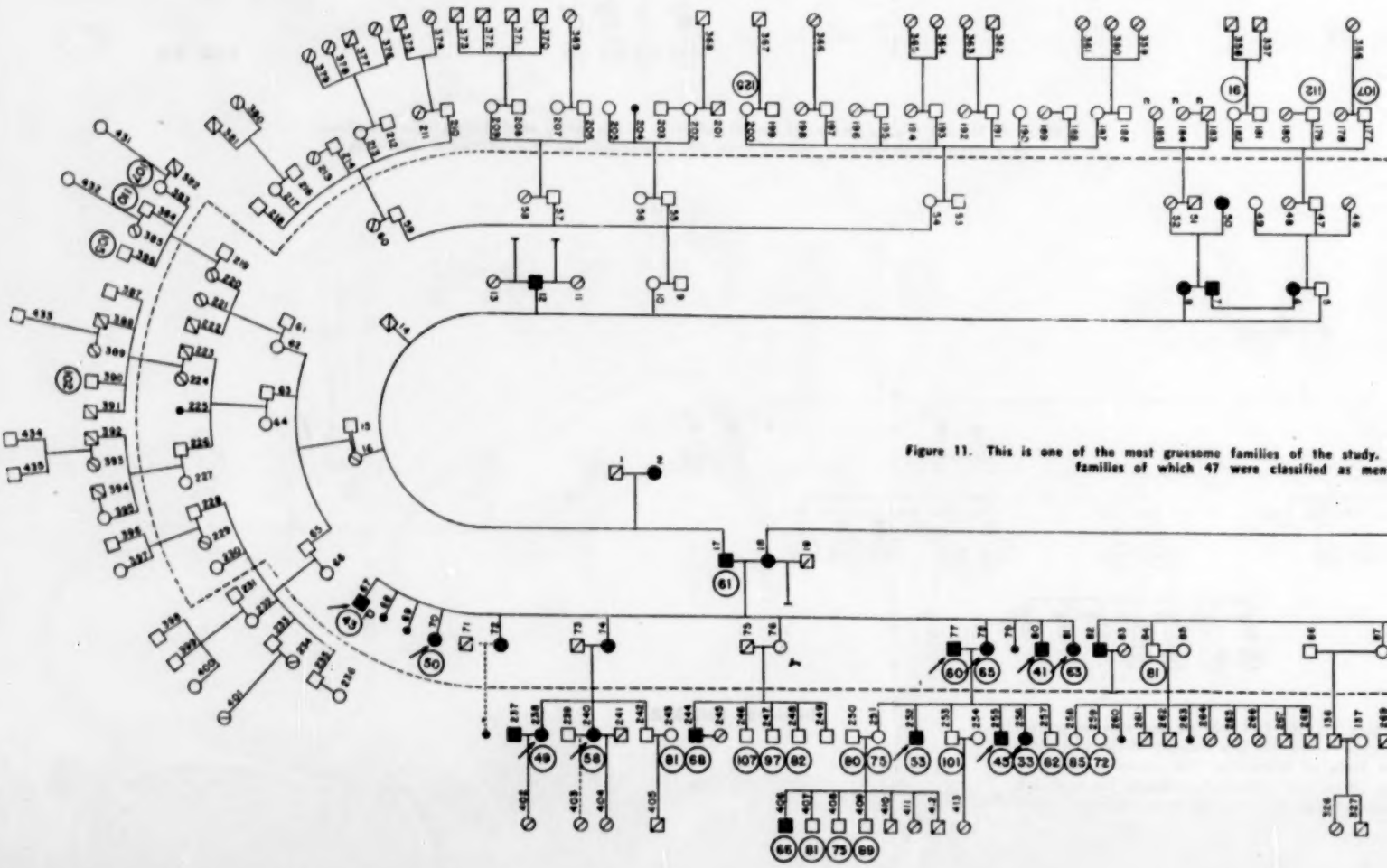


Figure 11. This is one of the most grussome families of the study.
families of which 47 were classified as men

I

ance of a pair of identical twins with microcephaly
(130) with the same defect.

KMD 133

VI

V

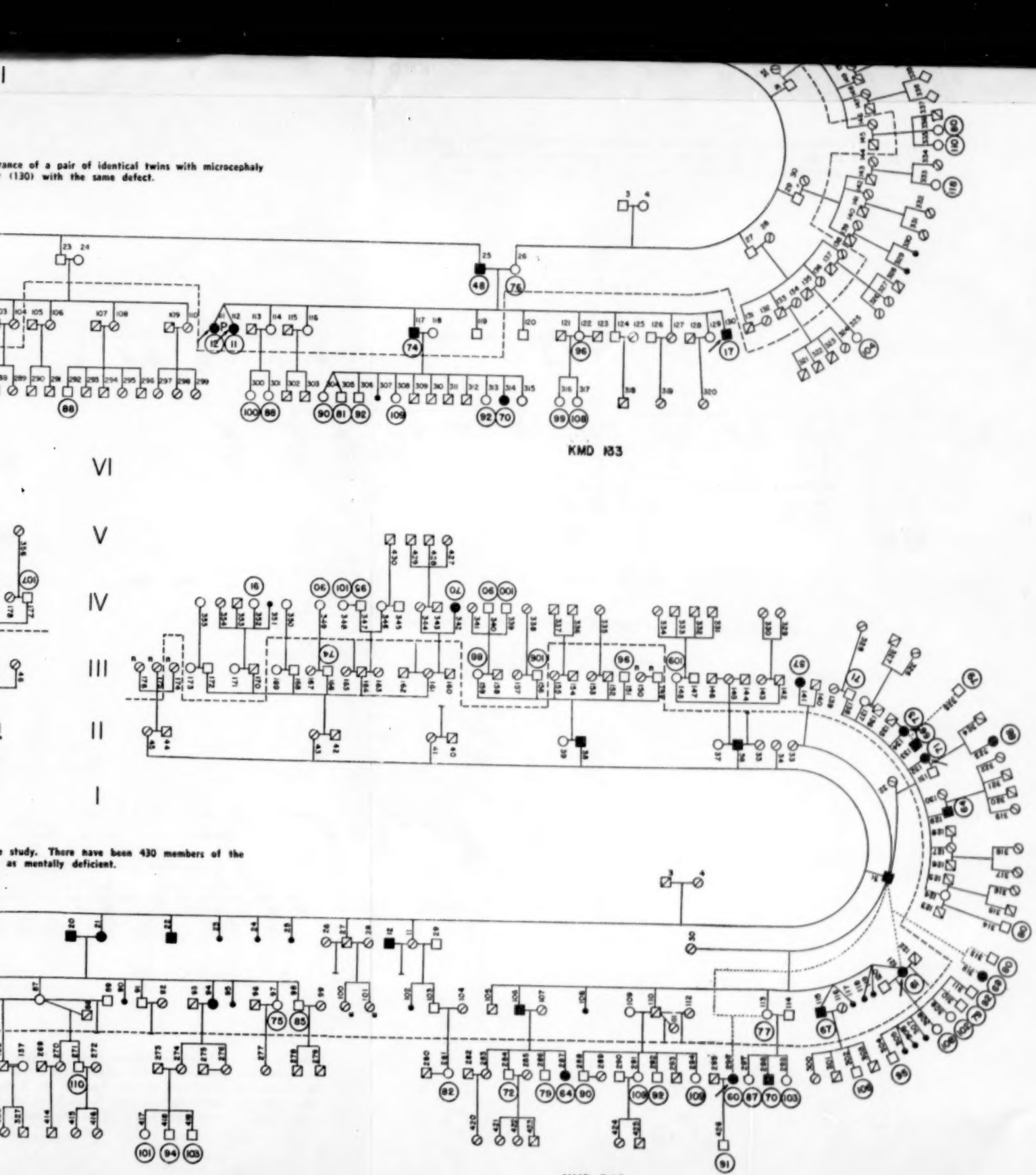
IV

III

II

I

study. There have been 430 members of the
as mentally deficient.



KMD 342

was available. The double bond connecting numbers 5 and 6 indicates a consanguineous marriage, in this case first cousins. It is assumed that the two very low grade mentally deficient children resulted from the homozygosity of a recessive gene inherited from a common ancestor as a result of the consanguineous marriage of their parents.

The first two figures showed families which became extinct. No new data were expected from them, as they were known to have come to an end at the time of the original study. Figure 3 (KMD 95) is a different story. There were a number of people in this kindred at the time of the original study, but since then they have just disappeared. The "U" under the symbols indicates that the person could not be found and is lost to the study. Undoubtedly a number of the members of generation III had children and grandchildren, but we have no clues as to where to find them. This family is our greatest failure. Fortunately no other kindreds this large were completely hopeless.

Let us leave the extinct and the missing families and proceed to the better ascertained and more interesting kindreds. Figure 4 (KMD 339) is a very interesting family. There are two kinds of dotted lines on this pedigree with entirely different meanings. The dotted lines connecting a male and female and then dropping to a child indicate that the child was illegitimate. It is rather ironical that the mentally deficient person (5) was herself illegitimate and produced two illegitimate mentally deficient children, but finally, after taking on the sanctity of marriage with (7), failed to produce any more children, as shown by the very short horizontal line at the bottom of the 5 to 7 union lines. The second type of dotted line is the long horizontal one which should always be looked for, as it divides those people born by the time the original study was done from those born since then, that is, during the last forty years.

The half sister of the proposita produced a varied brood of 10 children. Number 12 was given an I.Q. test in 1924 by Miss Mildred Thomson on which he scored only 35. However, it was recognized that he was deaf, so he was sent to the State School for the Deaf. He made fair progress at the deaf school and upon discharge from there acquired a wife and four children, of which the eldest (26) is now at the State School for the Deaf. He is not considered to be mentally deficient, so his square is not filled in. The next boy (14) has no hearing trouble, but is considered socially mentally deficient, and could only complete the sixth grade. The next three boys (16, 18 and 19) finished at least the first two years of high school. The next girl (21) was in the School for the Deaf for six years, but was found to be mentally deficient and transferred to the School and Colony for the mentally deficient, where she is at present. The remaining four children are not mentally deficient though the girl (23) was in an "opportunity room" until it was abolished. There is wide range of I.Q. values here, from 37, where deafness was also present, to 74 and 76 without deafness to a high of 118.

Figure 5 (KMD 454) is presented to show the rather remarkable number of still births. Of the four children in the sibship of the propositus, the other three died of "convulsions" at less than one year of age. One of the three was hydrocephalic and the proposita had spina bifida. However, she survived to be 54 years old and died of carcinoma of the liver.

The mother was syphilitic which may have accounted for some of the infant mortality, though the genes for the hydrocephalus, spina bifida, anencephaly syndrome are probably responsible for the mental deficiency.

Figure 6 (KMD 105) is a "Kallikak" type of family. Number 10 had an affair with (11) and felt it necessary to marry her though she had previously had an illegitimate child (34). The rest of the paternal

side of the family is highly respectable; number 15 was a judge. Number 31 escaped from the Institution several times before she accepted sterilization, and was then discharged. She married a mentally deficient scamp (30), and they then abducted her sister (33) from the Institution and took her to California with them, where they promptly abandoned her. The State of Minnesota had to pay the expenses of returning her to the Institution. However, as the abductors have been on relief in California for over thirty years now, Minnesota has not suffered from their departure. Furthermore, it is California's good fortune that the young lady (31) was sterilized before arrival.

Figure 7 (KMD 341) presents something of a problem in that three of the sibs of the proposita are now lost to the study. We know that numbers 18 and 22 had children, and it would be important to determine their intelligence. In the 29 persons of this kindred where the mentality has been established, 15 of the 29 were mentally deficient. There is a hint here that a dominant gene is being transmitted, which causes the mental deficiency. The original workers knew that number 3 was a brilliant merchant in Russia and the relatives in this country said that the grandmother (4) was normal. The grandparental generation is just beyond the line of reliability of ascertainment, particularly when the grandparents did not come to this country as was the case here. We suspect that perhaps the information from the relatives in regard to the grandmother (4) was falsified.

The family conditions were adequate and the parents over-indulgent to the proposita. The proposita had several physical examinations, the last was a thorough physical and neurological work-up by Drs. Böök and Schut in 1950. They spent much of one year giving such examinations to persons in this study. The proposita had many abnormal neurological reactions, but none of them could be selected as correlated with her idiocy. She is still alive, in general good health, and her Wasserman, Kolmer and chest X-rays were all negative.

So far we have considered only small families. Let's look at the largest one in the study, Figure 9 (KMD 163). There are five grandparents here as one of them (4) married twice. The five established a kindred of 494 persons. There were 16 persons of the 211 in the original study (internal to the dotted line) considered to be mentally deficient at that time, but these were mostly subjective determinations. We have found no mental deficiency in the 283 persons added in the recent generations (external to the dotted line). Furthermore, we are satisfied that two of those originally said to be mentally deficient were not so; one of them was the father of the only college graduate among the 494 persons.

This family is not interesting for its mental deficiency but because it is the largest family and because it is a part of the kindred which we have studied by reason of the 121 cases past, present or future, which it is expected to produce, of Huntington's chorea. We have presented an hypothesis, Reed and Palm (1950), suggesting that the gene for Huntington's chorea was in part responsible for the high fertility in the family.

The next kindred includes 419 members, Figure 10 (KMD 133), and is of considerable genetic interest. It will be noted that either (111) or (112) could be considered to be the proposita as they are identical twins, both microcephalic idiots. The last born of the twelve children in the sibship is also a microcephalic. Figure 12 shows the girls on the steps at Faribault about forty years ago, and Figure 13 shows them as they look today.

Considerable work has been done with this kindred. For instance, we have at least one I.Q. determination for no less than 68 members of the kindred. A careful physical and neurological study of the microcephalic twins, their microcephalic brother and other members of the kindred has been reported by Böök, Schut and Reed

(1953). In that publication the hypothesis was presented that while two "doses" of the gene for microcephaly produce the characteristic head shape and idiocy, one dose of the gene also lowers the I.Q. to that of the dull normal or moron level of intelligence. This is a reasonable hypothesis, whether here true or not, for many of the types of mental deficiency. Countless cases are known in laboratory plants and animals in which the effect of a single dose of a gene is greatly increased when it is present in the double dose. If a single dose of the microcephaly gene does lower the I.Q. significantly, then it might be expected to increase the fertility of those having it and thus account in part for the very large size of this kindred.

Both parents of our twins and two of the four grandparents must have been heterozygous for the microcephaly gene, and we can be certain that there is now a large number of persons, in the county in which the kindred lives, who are heterozygous for this pernicious gene.

The last kindred which we will consider in our brief survey is also a very large one of 430 persons, Figure 11 (KMD 342). We have at least one I.Q. value for 86 persons. Furthermore, in one family of the kindred we have I.Q. values for four consecutive generations, undoubtedly a record. Figure 8 is a condensed verison of the part of KMD 342 for which we have I.Q. values in four consecutive generations.

This is one of the most gruesome families we have come upon. One of the four grandparents was mentally deficient. There were 13 mentally deficient in the second generation, 19 in the third generation, 13 in the fourth and one in the still unfinished fifth generation, or a total of 47 mentally deficient. There would have been even more than this if the State had not carried out its eugenic function of institutionalizing seven of the mentally deficient by the time the first study was done and eight more in the subsequent years. However, the kindred is also a striking example of what happens when the State fails to carry out its eugenic function completely. For example, (31) is now an old man in his seventies who never worked. By his three wives he produced 17 children and three more by two of his mentally deficient daughters. Among his children and their offspring are 12 cases of mental deficiency, of whom only four have been institutionalized. The agencies of the government subsidized this man's venture in reproduction and society will pay for the results for some time to come.

The other remarkable man (17) in this pedigree (see Figure 8) produced 15 mentally deficient offspring, of whom nine have been institutionalized. Unfortunately, institutionalization usually did not come soon enough.

Eugenics fought a good fight with the genes for mental deficiency in this family but won only a partial victory. But let us not underestimate the value of the partial victory, not only to our society but also for the unfortunate individuals who would have been born in misery were it not for the action of the State in preventing their births.

Replacement Rates

When looking at a kindred such as that shown in Figure 11, the future seems to be composed mainly of black squares and circles. But that is the blackest kindred we have. Let us consider the whole picture rather than isolated kindreds.

We know that the 296 propositi, all institutionalized at some time, did not as a group anywhere nearly replace themselves. But what about the brothers and sisters of the propositi? Did they breed like rabbits? We should expect that there were many brothers and sisters. For the 54 completed families the average size of the sibship, including the propositus, was seven persons. In order for a sibship of seven to replace itself it must produce two children apiece or a total of 14 children. Actually we find that the production was only nine children, instead of the 14 necessary for

FIGURE 12. (KMD 133).
The identical microcephalic twins on the steps of the State School and Colony at Faribault about forty years ago.

FIGURE 13. (KMD 133).
The same twins, shown in Figure 12, as they look today. Their long life in the institution has been healthy and uneventful.

mere replacement. This is 0.65 of the number necessary for replacement and is quite a surprise. We will have to consider the complicated causes for this relatively low fertility of the sibs of the propositi elsewhere. Some of the more obvious causes may be mentioned, however. In the first place, there are often sibs that are affected with mental deficiency in addition to the propositus. Some of these sibs will also be institutionalized and the ones with very low I.Q.'s will not reproduce, even if not institutionalized. Secondly, the sibship of seven includes still births; none of them have any chance of reproducing, of course. Finally, as one or both parents of the sibship are sometimes mentally deficient, the accident rate resulting in fatalities among the sibship is very high. Furthermore, the presence of mental deficiency in the family may act as an eugenic brake and cause the normal sibs to limit their families.

It might be thought that the nine children actually produced by the seven members of the propositus' sibship is still a rather high rate of increase, as there would only need to be six persons in this generation to replace the genes for mental deficiency of the parents of the propositus. However, it should be remembered that some of the nine are still-births and others may be institutionalized, so that you can't tell till the next generation is produced how biologically effective the nine children will be.

Finally, when it is recalled that the nine children were born in Minnesota, while birth rates were still fairly high, and certainly well above replacement, it seems doubtful whether the genes for mental deficiency present in the parents of the propositus have held their own in relation to their normal alleles in the rest of the population.

Let us consider now the replacement rate of the whole kindred, including the sibship of the propositus. If we take the 54 kindreds that have been completed and reject from them the kindreds in which ascertainment is not complete because some aunts or uncles remained in Europe, or a section of the kindred in this country is lost to us, we find that the average size of the kindred for the cumulative first, second, third and fourth generations, including the people who have married into the kindred, is 158 persons where we would expect an average of 40 persons for exact replacement. The kindreds as a group, through the fourth generation and omitting the persons born so far in the fifth and sixth generations, are therefore almost four times as large as would be necessary for replacement. This does not impress us as being an unusually high rate of increase for kindreds started by persons born between 1800 and 1850 in a frontier state.

It is most interesting that the kindred as a whole had a higher reproductive rate than the sibs of the propositi, and that the sibs had a higher rate than the propositi themselves. If this finding is substantiated by the other five-sixths of our kindreds, it means that the more distant relatives of the propositi reproduced at a higher rate than the sibs of the propositi. It would seem that this shows that the normal genes reproduce themselves at a higher rate than the genes for our group of mental deficiencies as a whole. This conclusion is not that held by many eugenicists of the past. Perhaps their attention was restricted to those ghastly families, such as KMD 342, the last one illustrated, which are not representative of the average kindred where mental deficiency was present. But even in KMD 342, the longitudinal study shows that the mental deficiency genes are being eliminated in favor of the normal alleles.

The evidence from the part of the study so far completed indicates that the genes for the various kinds of mental deficiency behave like the genes for physical abnormalities. That is, they are eliminated from the population because of their failure to compete successfully with their normal alleles. There may be a few cases where the gene for mental deficiency when pres-

ent in the heterozygous condition gives a reproductive advantage, as we have assumed, for microcephaly. However, even here there must be some brake upon the spread of the gene, or it would be more frequent than it is.

On the whole, however, the genes for mental deficiency must be inferior to their normal alleles and are being continuously eliminated, only to be replaced by new mutations to the same abnormal genes. The eugenic activities of the state assist in the more rapid elimination of the abnormal genes. The most exciting discovery that the eugenicist could make would be to find a way of preventing the remutation of the genes for the various kinds of mental deficiency.

Summary and Conclusions

In 1949 the Minnesota Human Genetics League initiated a follow-up study of an investigation of the intellectual and reproductive capacities of the families of the mentally deficient. The original work took place between 1911 and 1918 with the families of a selected sample of 549 patients at the Minnesota State School and Colony in Faribault.

A drastic subsequent selection from the original 549 family histories was made for the follow-up study. Two hundred and fifty-three histories, mostly of epileptic patients, were set aside, leaving 296 families for the present study.

The intelligence quotient for all but six of the 296 patients was taken by the original workers and the average I.Q. = 38.

A very low fertility was expected for the propositi because they were all institutionalized for some part of their lives. Only 37 (12.5 per cent) were known to have had any children. They had an average I.Q. of 48 while their 80 children had an average I.Q. of 71. If we average the 80 children over the 296 propositi, we have an average of 0.27 children per institutionalized mentally deficient subject.

In the present study the four grandparents of the propositus and all the descendants of them are included. The average size of the 54 families so far completed was 158 persons. This is about four times the number of persons necessary for exact replacement of the genes of those involved in a family structure stemming from four grandparents and extending over four generations. This does not seem to be a particularly high rate of increase for families from grandparents born between 1800-1850 and residing in the frontier state of Minnesota.

For the 54 completed families the average size of the sibship containing the propositus was seven persons. In order for a sibship of seven to replace itself it must produce a total of 14 children. Actually the production was only nine children, instead of the 14 necessary for replacement. This is 0.65 of the number necessary for replacement and is quite a surprise.

It is most interesting that the kindred as a whole had a higher reproductive rate than the sibs of the propositi and that the sibs had a higher rate than the propositi themselves. If this finding is substantiated by the rest of the study, it means that the more distant the relatives from the propositus the higher the reproductive rate. It would seem that this shows that the normal genes reproduce themselves at a higher rate than the genes for our group of mental deficiencies as a whole.

We conclude that the genes for severe mental deficiencies behave in the same way as do the genes for severe physical defects. They tend to be gradually eliminated from the population and presumably are replaced by new mutations from normal genes.

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III. INTELLIGENCE IN FAMILY GROUPS IN THE BLUE RIDGE MOUNTAINS

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ATTEMPTING TO EVALUATE genetic factors in "intelligence" in man is a little like trying to catch smoke in a basket. First, we have no really satisfactory method of measuring the attribute which we wish to assess, and second, this attribute is certainly a resultant of many factors. Therefore any attempt to investigate any one or even several of the contributing factors must admittedly be only an approximation and probably will include errors the magnitude of which can only be guessed. In spite of these limitations, however, studies in this field should be of particular interest to geneticists. The predominance of man as a species is largely due to the high degree of development of his nervous system and his ability to think, and the evolutionary significance of man's mental ability is tremendous. Any study that may enable us to understand the functioning of this ability and perhaps throw light on the hereditary factors which establish this species characteristic, and which apparently set the limits of its functioning ability, therefore seems worth-while.

There have been numerous studies of the ranges of intelligence in various population groups, as measured by one or more of the standard intelligence tests. There are also numerous evaluations of the validity of test scores, advantages and disadvantages of various types of scores, and discussions of the effects of a wide range of environmental variables on the test score results (Woodworth, 1941; Wechsler, 1944). There seems to be general agreement that the standard form intelligence tests measure both an hereditary component of basic intellectual ability and certain modifications of this resulting from learning and experience. For this reason comparisons of test-score results between populations with differing opportunities for education and experience must be made with caution.

The distribution of intelligence in the general population approximates the "normal" or Gaussian curve, and indeed the standard test-scoring methods have been standardized on this basis. We have come to expect continuous variation with this type of distribution in practically all normal metrical characters which are determined largely or partly by genetic factors. Such distributions are in accord with the theory of multiple genes as determining factors of the base pattern, with modification to a greater or lesser extent by environmental factors. The multiple gene theory implies that the basic limits of reaction or capability of the individual are determined by a constellation of genes, while experience and education may greatly modify the relative position of the individual within the limits of this group. The extremes of the normal distribution, the gifted and genius groups at the high extremes and the borderline and high-grade mentally deficient groups at the low extremes, represent the rarer fortuitous combinations of the available genes offered by the parents. An exception to this must be made in the case of low-grade mental deficiency, which does not fit into a scheme of continuous variation (Kallmann, 1953), but represents a pathologic suppression of the genetic potential for normal mental development, the suppressing factor itself being sometimes genetic and sometimes environmental in origin.

As yet we have no critical data concerning the number of genes that may be effective in determining normal intelligence in man. Most of the schemes that have been suggested hypothesize a relatively small number, such as the suggestion of Hurst (1934) of one major and five minor gene pairs. Evidence of the importance of hereditary factors is provided by twin studies, which uniformly show high degrees of correlation in I. Q. scores between pairs of monozygous twins, and lower correlation between pairs of fraternal twins and sib-pairs. Newman, Freeman and Holzinger (1937) found the correlation coefficient with respect to the Binet intelligence quotient for monozygous twin pairs to be .88, while that for dizygous pairs was .63. Herrman and Hogben (1933) found the mean difference in I. Q. as measured by the Otis advanced test to be about half as great in monozygous twin pairs (9.2 ± 1.0) as in sib pairs (16.8 ± 2.3). Correlations have also been studied between parent and child, usually being found to be somewhat less than .50, but higher than correlations studied between parents and adopted children. Several studies on twins and within family groups have been summarized and evaluated by Woodworth (1941) and by Osborn (1951). Kallmann (1953) summarizes the situation by stating, "According to the most recent authoritative estimates, the genetic contribution cannot be assumed to be less than 50 per cent or more than 75 per cent of the total measured variation of normal or moderately deficient intelligence."

An additional point relating to the distribution of intelligence levels in the general population that is puzzling and for which no adequate biologic explanation exists has been pointed out by Roberts (1945). Based upon the very large samples studied by the Scottish Council for Research in Education, it is shown that while the mean scores of boys and girls are essentially equal, the variance is much greater in boys, the difference in the two variances being 15 times its standard error. This indicates that the scores of girls tend to cluster more closely about the mean, while a larger proportion of males are found at the extremes of distribution, both among the most highly intelligent and among the dullest groups. The significance of this observation is unknown.

We have recently been interested in studying the distributions and intra-family correlations of certain characteristics in a normal population in western North Caroline, the major purpose being to study factors involved in selective mating. The area under study was Watauga County, located in the Blue Ridge Mountains, a part of the Appalachian chain. The county is predominantly rural, with no heavy industry, and with a population in 1950 of 18,342. Although the area is mountainous and the more remote sections might be considered rather isolated, the county is traversed by a system of good roads providing access to surrounding areas. The town of Blowing Rock is a tourist and resort area, and a state teacher's college is located at Boone, the county seat. The area was chosen for study as being typical of this mountain district.

There were scattered settlements in the Watauga area before the American Revolution, and practically all arable land was taken up during the eighteenth century. The original population was predominantly of English descent. The population has been relatively stable, a large proportion of the present inhabitants being descendants of the original settlers. During the nineteenth century there was a steady stream of emigration from this region to the lands then being developed in the western states. Migration into Watauga was considerably less, with no major trends of origin being apparent. Both the roads and the primary schools were rather poor until after World War I, but transportation and education have improved markedly during the past 25 years.

An attempt was made to study a fairly

wide range of normal characteristics, including educational background and intellectual ability, in a series of randomly selected families resident in Watauga County. Families were selected by making a random choice of houses on rural roads, and asking the occupants to cooperate. These choices were so arranged that the number of families studied in each township of the county was roughly proportional to the population of the township according to the 1950 census. About half of the families approached in this manner declined to cooperate, but no definite trend with regard to social or economic level or family size was noted in the refusal group. As the battery of tests desired usually required two or three days' study for the average family, we feel that the rate of cooperation attained was satisfactory. There was no special motivation involved in acceptance of the study by families, as we offered no special services or compensation for cooperation and had no sponsorship other than the good name of our school of medicine and its teaching hospital.

Of all of the factors chosen for study, it is unfortunate that the most incomplete data are those provided by the intelligence tests. The clinical psychologist member of the field team was recalled to military duty during the course of the investigation, and our family data in this respect are therefore sparse, including data on only 86 families. The Wechsler-Bellevue intelligence test was used, with appropriate use of alternate forms where indicated. In each household, an attempt was made to obtain an I. Q. score for the husband, wife, and all children. Data on some families remain incomplete. For example, we were able to test more wives than husbands, and we were often unable to test older children who had left the family circle to work or live elsewhere.

The distribution of I. Q. scores for the entire sample of 223 individuals was found to approximate the expected normal distribution, with an average of 94.52 ± 0.87. The average is thus slightly below the standardized mean of 100 as determined on the New York population. As the background of experience of the rural mountain group is considerably different from that of a metropolitan urban group, we are not convinced that the observed mean really is significant of any true difference in basic intellectual ability.

In 49 families we were able to test both the husband and the wife. The differences in scores between members of a mating pair are given in Table 1. It will be noted that in approximately half of the marriages, the agreement in I. Q. score of husband and wife is within 10 points. In only 3 marriages was there a disparity of more than 20 points in the scores of a married couple. This small sample tends to bear out the observation frequently discussed of intellectual ability as a factor in mate selection generally.

TABLE 1

DIFFERENCES IN I. Q. SCORES OF HUSBAND-WIFE PAIRS

Difference	*Frequency*
0–4 points	12 marriages
5–9 "	12 "
10–14 "	9 "
15–19 "	11 "
20–24 "	2 "
25–29 "	0 "
30–34 "	1 "
TOTAL	49 marriages

In the entire sample, the mean scores of all husbands and all wives were not significantly different, being 96.17 ± 1.72 for husbands and 94.75 ± 1.97 for wives. Within families, correlation coefficients were calculated for husband-wife pairs, mother-child pairs, father-child pairs and sib pairs. These results appear in Table 2. The highest degrees of correlation were found between husband and wife and between sib pairs. The correlation between mothers and their children was less, but still significant. This sample is unusual in failing to show evidence of correlation between fathers and their children. Other studies

have consistently shown a higher degree of correlation of the scores of children with those of the mothers than with those of the fathers, but a significant correlation in father-child pairs is consistently found. We can only attribute this unexpected result to small sample size and sampling error. This is probably related to the fact that the small sample of fathers had the highest mean noted for any group, this being 101.07 ± 1.64, while the average score of children was 94.10 ± 1.33. The fact that all "husbands" were not also "fathers" accounts for the discrepancy in the means cited for these two groups.

TABLE 2
CORRELATIONS WITHIN FAMILIES IN I. Q. SCORES

	Pairs	*Correlation*
Husband-wife	49	.40 ± .14
Mother-child	80	.24 ± .11
Father-child	49	—.02 ± .14
Sib-sib	62	.45 ± .13

Among the adults tested, the sample was examined for evidence of a secular trend. From the known history of primary and secondary school facilities in the area, it seemed that the younger adults had somewhat better educational opportunities than the older ones. Among adults there was no significant correlation of test score with age. The possibility of correlation of test score with birth rank was also tested for the children, and no significant deviations were found.

In summary, the results of this relatively small sample provide no new interpretations, as they agree in principle with other published studies, but they do provide some information on a segment of the population for which few data are available. In some quarters it is a rather popular notion that the southern mountaineer is a lazy, shiftless and stupid character, this characterization being due in no small part to "hill-billy" cartoons, songs, and stories. We regard this notion as nothing short of slander. Although there are certain local customs and figures of speech that may seem strange to the casual visitor, the same is true of almost any definable population group. Our data indicate a normal range of intelligence among natives of the Blue Ridge Mountains, with a mean not greatly different from those observed in other rural areas.

A comparison may be made with certain other segments of the North Carolina population. Jordan (1947) has reported the result of group tests done on 1,980 white school children in another rural county (Orange County) located in the central part of the state in the industrial Piedmont area. The mean score on the Pintner series of tests for white children was 91.40, this being slightly less than the corresponding mean of 94.10 observed in Watauga. To illustrate the consistent advantage enjoyed by urban groups in tests of this sort, the same group (Jordan, 1948) tested 5,429 white school children in Winston-Salem, a city also in the Piedmont area with a 1950 population of 92,477. The mean score of this urban group was 97.70. The reservation should be made that careful individual testing, such as was done in the Watauga sample, is generally more accurate than group testing, with the group tests sometimes giving values lower than those obtained by individual scoring. Caution should therefore be employed in comparing our figures with those of Jordan, but even with all allowances our rural mountain group shows no remarkable difference when compared with other rural groups.

A further point that may have some interest in this connection is the unusually high rate of cousin marriages found in the Watauga population. A preliminary report on cousin marriage rates in this population was presented at the 1952 meeting of the American Society of Human Genetics but has not been published. This report was based on pedigree analysis of 1,651 marriages, and indicated a first cousin marriage rate of 1.57 per cent. Marriages of persons related as close or closer than second cousins (coefficient of relationship 0.0312 or

higher) make up 6.72 per cent of the total marriages. It is difficult to find strictly comparable figures on other normal population groups as most studies report only the incidence of first cousin mariages. Data summarized by Stern (1949) and by Neel *et al.* (1949) give first cousin marriage rates in normal American and European populations ranging from 0.20 to 1.03 per cent. The rates determined in the Watauga sample are two to three times as high as the usually reported figures. This would tend to reflect relatively small sizes of mating isolates, a finding not unexpected in a rural mountain area.

It is a rather common misconception that a high cousin marriage rate and the resulting rise in level of inbreeding of a population should be associated with prompt deterioration in both physical and mental abilities. Dahlberg (1947) discusses this point in some detail, and the following quotation is pertinent. "It is not infrequently asserted that the defective individuals in the isolates and the primitive cultural conditions are a sign of degeneration due to inmarriage. This is wrong, of course. There is no reason for assuming that the hereditary constitution of the population of small isolates is either better or worse than in large isolates." We feel that our data support this position, as they indicate a range of I. Q. scores within normal limits for a rural population, occurring in a population with an unusually high cousin marriage rate and with presumably small size of mating isolates.

Summary

1. Data are presented concerning the distribution of intelligence quotient scores obtained with the Wechsler-Bellevue test on 223 persons in 86 families selected at random from the population of Watauga County, North Carolina, located in the Blue Ridge Mountains.
2. The mean I. Q. score of the sample was 94.52 ± 0.87, this being within the range of normal reported for other rural populations and consistent with examinations of larger groups from other segments of the North Carolina population.
3. The intrafamily correlations are consistent with usually accepted estimates that the genetic contribution to the total measured variation of normal intelligence is not less than 50 per cent nor more than 75 per cent.
4. It is pointed out that the population studied provides an example of a normal distribution of intelligence quotient scores in a group with an unusually high rate of cousin marriages and presumably with small size of mating isolates.

Acknowledgment: This report is based in part on data collected by Dr. Harold Fahs and E. R. Kerley, and analyzed by Dr. H. O. Goodman.

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MARRIAGE AND FAMILY RESEARCH

A Critical Evaluation

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RESEARCH tends to keep marriage and family educators modest in the assertions they make in their teaching and counseling. One of its major functions is to verify or refute such assertions, indicating for what populations they are true and under what conditions. "How do we know" should be asked whenever we are encouraged to advance our ideas as truth.

Of the twenty studies of marital happiness summarized by Clifford Kirkpatrick in "What Does Science Say About Happiness in Marriage," 152 different factors were found to be significant in at least one study, but the agreement among these twenty studies was very small on any one of these factors. One factor—happiness of husband's parents' marriage—was supported by five studies; two—length of acquaintance and duration of engagement—were supported by four studies. More recent research, using tests of marital adjustment developed by Ernest Burgess and his associates, show more agreement in this respect, although this research has been carried out on Southern Negro couples, Swedish couples, and Chinese. Some research on more mature families suggests that marriage adjustment reflects different factors later in the family cycle and that the marriage texts are teaching what makes for happiness in the first few years of marriage rather than for the entire family life cycle.

Research, therefore, is useful to test the common-sense generalities which pass for fact only because marriage experts have written them up in books. Much of what we think is vigorously denied by students taking another approach.

Research also serves to deal with some of the pressing problems raised by the public, but few family sociologists enjoy attacking these problems. Relatively few researchers have directed their energies to get answers to these highly personal questions; Judson Landis, Jessie Bernard, Harold Christensen, and Winston Ehrmann are among the exceptions. For example, the public asks questions such as these:

1. How to meet and marry?
2. How to marry happily; how to remain happy—can second marriages be happy?
3. How to plan a family; how many children in an ideal family—Does pregnancy improve marriage?
4. What can families do together?
5. How to understand children and be good for them.

Of much more appeal to research has been a third task, that of filling in the gaps of family theory dealing with the following:

1. The social psychology of marriage and family interaction
2. The interrelations of family structure and personality growth
3. The interrelations of the family and other social structures
4. The interrelations of types of marriage and family forms and their success in meeting the needs in family living.

Such research leads us to existing new knowledge about the family as a small

[1]Abstracted from a speech given at Columbus, Ohio, April 1953 at the 16th Annual Groves Conference on Marriage and the Family

group and to new insights as we go from the known to the unknown. And it hints at a common science of human relations, as we attempt to generalize findings from the family as a small group to groups other than the family.

Trends in Research

The first family research dealt discreetly and expediently with far-away families because to have studied families at home would have been hazardous, a violation of the sanctity of the home. In our own day the pioneers of family research have seen this area of investigation shift from being suspect to becoming respectable.

Coincident with the shift to respectability —for it takes less courage to do research in marriage and the family today—we find fewer of the prophetic deviant personalities in the field and more pedantics. Two new arrivals, Kolb and Foote, have what Becker would call "charisma," the desire and capacity to act as image breakers; most of the other researchers are image worshipers.

Sheer numbers of research projects in the family field have increased 700 per cent from 1942 to 1950, but the number has dropped almost 50 per cent since then. The focus of marriage and family research has made striking shifts since the last decades of the last century from large-scale comparative analyses of great sweep like Westermarck's study of the history of marriage forms and similar studies of ancient family customs to present-day microscopic explorations of the internal workings of families in our home neighborhoods.

The shift from the study of matrimonial institutions to the families in which people live has been gradual over the past twenty years. Ernest Burgess is credited with launching the movement to consider families as living associations in the late twenties, but its development in the form of concrete research projects ha: had to wait upon the willingness of individual families to be studied, the emancipation of scientists from blinders imposed by our mores, and the formulation of research methods for collecting, analyzing, and interpreting the new family data. The most recent survey of the National Council on Family Relations shows only 4/104 projects concerned with the institutional aspects of the family. The balance concentrated upon the effects of family relationships on personality development, critical analyses of courtship, marriage and family practices, and the effectiveness of teaching programs in preparation for marriage and parenthood. The shift is quite complete.

Another trend in family research is the movement of national leadership to bring about coordination of research efforts; most of our family research has been random, scattered and subject to faddish imitation. The National Council on Family Relations in 1945 invited research workers dealing with the effects of the war on marriage and family relations to share their research plans and findings with one another, and a division of labor was agreed upon. In 1950 the National Council in cooperation with the Committee on Human Development of the University of Chicago carried out a multi-disciplinary workshop on marriage and family research to train researchers from eleven different disciplines in a common language, stressing family development and structure-function approaches to family study. The Family Research Committee of the Social Science Research Council has financed an intensive critique of family research and charted directions for future research to follow; its findings will be published soon.

Finally I think we should recognize that marriage and family research is beginning to show evidences of cumulative generalizations and theory, that it is becoming accretive. By that I mean the specifying in each study of the hypotheses from previous studies that are being tested. As illustrative a few may be cited.

1. Strauss, Winch and Mangus in their courtship and mate-selection studies have tested and only partially con-

firmed the ideal-mate and parental-image hypotheses of Freudian family sociology.
2. Kirkpatrick and Caplow have tested and do not confirm Waller's theory that breaking of a courtship relationship is traumatic, like a divorce, for the party who cares more.
3. Burgess and Wallin have tested hypotheses advanced in the classic study by Burgess and Cottrell—that the same factors make for success in engagement that makes for success in marriage.

Similar in type is the trend to test findings from one region of the country and to test generalizations made for one social class in other regions and social-class groupings. Ruth Cavan's new text, "The American Family," has taken full advantage of this research to give us at long last a picture of the American family which specifies the variations in family patterns, practices, and objectives by the major regions, social classes, and different ethnic configurations in the United States.

Contemporary Emphases

Family sociologists are currently doing almost all the research reported on mate selection, courtship, marriage, institutional aids, and on the family as a social institution, if our sources are at all exhaustive. They are also appreciably more concerned with the operations of the family as a small-group association. The research reported as mate selection and many of the marriage research studies are predominantly validating in character, verifying theories of propinquity, of endogamy-exogamy, and of complementary needs as they operate in mate selection. There are obvious gaps and discontinuities observable in the research in progress in any given year which may be remedied in subsequent years. Nevertheless, I am tempted to point to the apparent discontinuing of significant lines of research which have been carried on for years and now seem to have slowed down, at least temporarily:

Studies of the societal contexts of the family
Historical development of family forms
Cross-cutural analysis of family patterns
Family-expenditure patterns and family values
Impact of technology and social change on family functions
Sex and the family
Divorce and postdivorce adjustments
Family adjustments to bereavement

The quick overview of research under way and the trends cited are heartening, but critics have rightly expressed impatience with our modest results to date.

Cottrell and Foote in their recent monograph, "New Directions of Research on the American Family," come closer to our major deficiencies than any other critics I have read. They make these criticisms:

1. Our research is scattered; several hundred articles are printed about some problems, almost none about others.
2. Our research is often amateurish, reflecting its source in masters' and doctors' theses, too often done by one-shot researchers, limited in scope and rarely followed up.
3. Most research is limited to sheer description; fact finding prevails over theoretical speculation, formulation of hypotheses, and the design of decisive experiments. The principles derived from the observations are of a very low level of generality, the analyses not very deep or discriminating. Even the concepts employed are rudimentary and isolated, rather than bearing systematic, deductive relations to each other. The development of family studies marks a history of styles in concepts rather than cumulative progression of factual knowledge, theory, and method.
4. Studies suffer from the penchant that family researchers have developed of viewing families as if they were "spectators" in a specious attempt to be objective; this gives a flat two-dimensional quality to much of the scientific writing which makes it dull by comparison with lay writing, uninteresting because the researcher is uninterested. The "spectator for sake of objectivity" role, moreover, has created a gulf between the family researcher and the social worker.
5. The spectator role accounts for the fact

that most research stops short of application, putting emphasis upon the pursuit of understanding for its own sake, and being satisfied with plausible *ex-post facto* explanations and imputations of findings. The practical result is that extensive tissues of speculation survive indefinitely amid rival systems of equal plausibility.

6. Family research has been limited for a number of years by its own value framework. Kolb has already pointed out in two articles that we have a value framework which emphasizes family stability as the major end of family processes. To be in equilibrium, or in adjustment, is the end in view. Research has been directed at the factors disturbing equilibrium and at the attributes which make restoration of equilibrium less likely which if removed or changed would enable the marriage or family to return to equilibrium, as if that were normal functioning. The results are to make patterns of conventional appeasement and harmony at any cost appear desirable for success in marriage. In this instance research has performed a disservice both for counseling and education.

Cottrell and Foote have used harsh words in referring to family research as scattered, random, meager, dealing with trivial problems, amateurish, dull and speciously neutral. But these criticisms are little more than dramatically worded readings from yardsticks which could be used to assess the adequacy of research in any field. The question we must ask is this: Are we more competent in these respects than we were twenty years ago, ten years ago, or last year? In sheer numbers of projects reported we have dropped in family sociology since 1949 from second to fourth place among the fields of sociology. Industrial sociology and population studies have surpassed us in attracting the energies of sociological scholars. In most of the other areas we could make a case for continued improvement in our competence, if not from year to year at least from decade to decade. We have labored under conditions unfavorable to research and have struggled with obstacles met by few other disciplines.

What has slowed up our progress in family research? First, families have resisted being studied. We have had to wait to share the sanctity of the home; the family is a closed system. However, the fact that each family is a closed system means the data collected will be more likely to have integrity and reliability than data collected from more adventitious associations. Family research has had to wait upon the willingness of individual families to be studied and the motivation and training of scientists to study them. Once this has happened, rich ores have been uncovered.

More frustrating has been the lack of methodological tools to collect, analyze, and interpret data obtained from families. Most rewarding have been the various forms of case study, the use of family histories, diaries, autobiographies, family budgets, and in recent years the discerning and focused interview techniques. These devices are best designed for collecting such data, but we are handicapped when it comes to analyzing and summarizing these data. Statistical methods have also been applied by students collecting census-type data. The statistical data lend themselves well to analysis but do not too often add materially to our insights; they deal largely with surface phenomena and are most useful in providing measures of the reliability of any finding. Prospects for speeding up progress in family research are dim unless the strengths of the case study and statistical methods are combined and their weaknesses ironed out cooperatively. The trends toward multidisciplinary research hint that this may be in the offing.

Two obstacles to progress in family research have been linked together: prima donnas and the discipleship developing around them. Our system of graduate study, personal by-lines in publication, promotion based on individual publication—all these have rewarded the solitary worker.

Several competing approaches for studying the family have grown up within sociology, social anthropology, psychology, and home economics, which provide relatively incompatible systems for viewing the family. Generalizations made by one approach cannot easily be added to generalizations made by other competing systems.

My associates at Iowa State College and the University of North Carolina have identified a number of systems competing for adherents among research workers today:

1. Zimmerman of Harvard asserts his institutional and historical approach is the only way to study true family sociology.
2. The interactionists are locked in battle with the structure-function advocates to see who will win the younger family researchers; both are able to focus upon the family as a small group successfully, whereas the Zimmerman approach has too broad a sweep to catch the conjugal family unit.
3. The learning theory-developmental approach makes the personality of the growing child the focus and perceives the family as the context of early learning, of socialization, guidance, and value orientation. The family is the setting, not the focus of study, and interpersonal relationships between father and child, sibling and sibling, and between husband and wife are slighted if treated at all. The family for these students appears to consist of a mother and a child without father or siblings.
4. The traditional home economists have applied the technology of management which deals with resources of time, money, and energy but until recently have skirted family values and family relationships in their research. Out of over a hundred experiment-station bulletins by home economists and rural sociologists supposedly studying family living, I found almost no mention of children, husband-wife concerns, or family activities as a family. The studies concentrated on expenditures and levels of living, facilities, standards of performance, use of time, nutritional standards, housing, health, etc. The conceptual framework of research home economists is more closely allied to farm management than family operation; it is really household management. Consequently it is hard to add their findings to any other propositions about the American family. However, no discipline is going family-centered more rapidly today than home economics; there have been tremendous gains in the last ten years.
5. The learning-theory-developmental approach is the dominant system among the projects reported by non-sociologists. Among family sociologists the interactional-role analysis approach most frequently characterizes the conceptual system used for defining and ordering research design.

With so many competing approaches to the study of the family, is it any wonder our findings don't add up to any consistent body of theory?

Finally, we are lagging in family research because there are almost no full-time research workers. Precious few men have undertaken to play the role of critics and synthesizers: Burgess, Cottrell, the Mowrers, Nimkoff, Kirkpatrick. Only four to my knowledge have undertaken to question the value premises underlying our research: Foote, Kolb, Marsh, and Humphrey, the so-called axiological analysts.

Prospects for Future Research

I find it difficult to forecast the future in research. I hope we are going to change rapidly enough so that my forecasts will be understatements. One direction will be the study of the family as a decision-making and planning association. The methods developed by Bales of interaction-process analysis will be peculiarly helpful in capturing the processes of problem perception, breaking the problem down into its components, suggesting actions required, and allocating responsibilities for carrying out the actions. The method of experimental participation described by Foote shows how engaged couples can be involved in identifying their objectives, getting together on a joint marital career, and increasing their ability to

intercommunicate. Much of the work on small groups, planning councils, discussion groups, industrial work groups, combat teams, etc., provides both methods and concepts which can be applied fruitfully to marriage and family groups as action groups.

A second direction will be the study of the family as it interacts with other associations in our society. The methods for studying the impact of the occupational sphere on the family sphere developed at Harvard beg application in other family and society points of contact: the family—and the legal system, and the religious systems, and the property system, and government, and the class system, and the educational systems, and the system of helping professions such as medicine, social work.

A third direction will be the study of the family as an operating unit in fertility planning and control, in medical care and health education, and in the prevention and treatment of emotional disabilities. This focus of study will profitably follow developments in the first two directions just mentioned. Agencies are discovering that many fronts do not yield results when working directly with children, that it is necessary to work simultaneously through the family as an active planning and controlling unit in order that programs outside the family will prove fruitful. To work through the family, we need some picture of the actions and the action potentials of this singular group association. Herein lies the rationale for this direction of study.

As I close, I want to call attention to a multi-disciplinary approach to family study which emerged from the planning for the first White House Conference on Family Life. Family development as a multidisciplinary framework has borrowed the concept of stages of the family-life cycle from the rural sociologists, the concept of development needs and tasks from the child psychologists and human-development specialists, and has taken from the family sociologist and social theorist the concepts of age and sex roles, plurality patterns, functional prerequisites, and the family as an arena of interacting personalities. All these concepts have been fused fruitfully to produce a frame of reference for studying the internal growth and development of families from formation to dissolution as follows:

> The family may be viewed as an arena of interacting personalities each striving to obtain the satisfaction of his basic desires. Parents who themselves have urgent needs make most of the adjustments in the building of complementary roles between themselves and their children. If we hold the entire family in focus in dealing with this drama, we see that many of the normal tangles of members during the family's life cycle are due to fundamental incompatibilities of the diverse developmental strivings of siblings and parents at critical points of growth. There can be identified, using these concepts, seven well-defined stages of the family-life cycle each with its own peculiar sources of conflict and solidarity. . . . Each of these stages may be seen in three dimensions of increasing complexity: (1) the changing developmental tasks and role expectations of the children; (2) the changing developmental tasks and role expectations of the parents, both mother and father, in their capacities as providers, homemakers, spouses, and persons; (3) the family developmental tasks of the family *as a family* which may be obtained by integrating the functional prerequisites of survival and growth (provided by structure-functional theory of social systems) and implications for the family of the personal development patterns of children and parents (derived from developmental task theory).

As this approach to family study is rendered researchable by more rigorous definition of concepts and by methods of data collection which can provide analyzable data, the dynamics of family development may be tested over a family's life span with living families. The outcome of that research may some day produce a companion volume to Dr. Benjamin Spock's pocket book, "Baby and Child Care." It may well be called "Your Family from Wedding Day to Dissolution Day."

HEREDITY COUNSELING

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Discussions of problems in heredity counseling will be presented in each issue of EUGENICS QUARTERLY *by a geneticist.*

THE GENETIC COUNSELOR meets all sorts of problems, no two really alike, but many involving some family tragedy. Unfortunately, a rather gloomy prognosis must be offered to many individuals requesting advice concerning problems in heredity. It is therefore a pleasure to encounter a couple to whom a more cheerful prognosis may be given.

Mr. and Mrs. X were deeply concerned over the fact that both had relatives with epilepsy. Although they had been married for six years, they remained childless because of fear of having epileptic children. They consulted their family physician concerning this problem and were referred to the Department of Medical Genetics of the Bowman Gray School of Medicine.

An extensive family history was obtained by questioning Mr. and Mrs. X. Their family physician had advised them to obtain as much information as possible concerning their relatives, and they brought with them necessary family records and reports from physicians who had treated certain relatives. It was possible to construct a nearly complete pedigree of both families covering four generations.

Mr. X was next to the youngest in a family of eleven children, three of whom had died in infancy. Three brothers and three sisters were living and well. An older brother was a severe epileptic. His convulsive attacks had begun at the age of eleven years and had gradually progressed in severity. Mental deterioration occurred, and he became subject to violent rages. As he was dangerous to others during such attacks, he was committed to a state hospital for epileptics and was still a patient there at the age of forty-five. All of Mr. X's six normal brothers and sisters were married and collectively had eleven normal children. However, one sister had an only son who was reported to be "crippled." Information from his physician indicated that this boy had cerebral palsy that was apparently secondary to birth injury and probably not of the inherited variety.

There was no evidence of any pertinent abnormality in Mr. X's father, his paternal grandparents, or among any of the six paternal uncles and aunts or their descendants. However, two additional cases of epilepsy had occurred in relatives of Mr. X's mother. His mother had nine brothers and sisters, all normal, but one of the sisters had seven children of whom one girl had frequent and severe convulsions, was thought to be mentally retarded, and had died of pneumonia at the age of eighteen years. Mr. X's maternal grandmother had five normal brothers and sisters, but one of these sisters had nine children of whom one had convulsions and two others were mentally retarded.

In summary, Mr. X had three blood relatives with some form of convulsive state, these being a brother, a first cousin, and a first cousin once removed.

In the family of Mrs. X there were two relatives who definitely had some form of epilepsy, and a third with a questionable history. Mrs. X's mother, age fifty-eight,

had been subject to occasional convulsions for the past ten years and had more frequent spells of "blanking out" for a few seconds, diagnosed by her physician as the petit mal form of epilepsy. The mother had four brothers and sisters, one of these having died at the age of forty, but having previously had minor attacks suggestive of petit mal. Mrs. X had twelve brothers and sisters. A brother, age fourteen, was reported to have convulsive attacks and visual disturbances present since an accident two years previously. He had been carefully studied by our Department of Neurosurgery, and it was found that the convulsions were of the Jacksonian type, secondary to the accident, and definitely not hereditary.

In addition to the patients with convulsions, Mr. and Mrs. X were worried about two other items. Three of Mrs. X's brothers, her father and two paternal uncles had nystagmus, or "dancing eyes." This consisted of uncontrollable rapid side-to-side motion of the eyeballs, present at all times, and interfering with vision to a moderate extent, but with no other symptoms. Nystagmus is often a sign of disease of the nervous system, and Mrs. X thought that this might be connected in some way with the epilepsy. We were fortunately able to show that the nystagmus was of the congenital stationary type, which is inherited, but is not indicative of any serious nervous or mental disease and causes no disability beyond moderate visual disturbance. We were able to demonstrate that nystagmus was due to a dominant gene in this family but that Mrs. X did not possess this gene and would not transmit this condition to her children.

Mrs. X's last worry concerned "insanity," as one of her aunts had sustained a "mental breakdown" at the age of forty-five requiring sanitorium treatment. Her psychiatrist informed us that the patient had only a menopausal psychoneurosis and was not psychotic. Hereditary factors are not known to be of importance in this condition.

The list of family abnormalities, which we might call the debit side of the ledger, seems bad enough, but is not nearly so gloomy as Mr. and Mrs. X had feared. The clinical and pedigree data clearly show that the nephew with cerebral palsy, the aunt with the "mental breakdown," Mrs. X's brother with Jacksonian epilepsy, and her relatives with congenital nystagmus were of no significance with regard to the prognosis for children of Mr. and Mrs. X. They were assured that these conditions could be entirely disregarded. The only debit item of serious concern is the presence of epilepsy of an inherited type on both sides of the family.

On the other hand, it was noted that there were also factors which should be entered on the credit side of the ledger, although these were a bit more difficult to evaluate. First, both Mr. and Mrs. X seemed to be above average in general intelligence. Being one of the youngest in a family of eleven, Mr. X did not have the advantage of a college education, but in spite of this he had risen to a responsible position in a large manufacturing concern. He was superintendent of a manufacturing section, his position requiring special knowledge of cloth manufacture and of repair and maintenance of intricate machinery, and also required skill in personnel management of nearly one hundred employees under his direction. Mrs. X had completed college, was a school teacher, and was successful in her profession also. Further inquiry revealed that both Mr. and Mrs. X were active in civic and church affairs in their home community and were well liked by their neighbors and associates. Physical examinations and inquiry to their physician demonstrated that they were both healthy physically and mentally. In summary, both seemed to be above average in those qualities which are difficult to measure but which are generally considered desirable in the community.

At this point the consultant felt that the debit and credit values were very nearly in balance for this couple, and more exact information seemed highly desirable. It is known that in many cases, though not in all, asymptomatic carriers of the inherited predisposition to epilepsy may be identified by use of the electroencephalograph. This instrument records the electrical activity of the cortex of the brain. Many carriers will show specific abnormalities of these records that can be identified. If either Mr. or Mrs. X could be definitely identified as a carrier of hereditary epilepsy, the risk that this disability might appear in their children would be of serious concern. While a normal brain tracing would not entirely rule out the possibility of this unfortunate event, it would indicate a much lower risk. The Neurology Department made the required examinations and reported that Mr. and Mrs. X both had normal tracings.

We therefore felt that the likelihood that epilepsy might appear in one or more children of this couple was quite small and that the other desirable qualities that they exhibited would far outweigh this risk. Mr. and Mrs. X were therefore advised of these opinions and reassured concerning their fears. We hope that they may have as many children as they desire.

This case is described in such detail to emphasize a principle that must always be kept in mind by the genetic counselor. One must not only consider the obvious abnormality which prompts the patient to seek genetic advice; one must also take into account all evidence that may be obtainable concerning the total genetic potential of the parties concerned. It is possible to lay down hard and fast rules for only a few very severe hereditary abnormalities, which are fortunately rather rare. Each case must be handled as an individual and unique situation, with due attention to many factors. The absence of exact and objective methods of measurement makes any estimate of total genetic potential a matter of judgment, and in addition such factors as the mental stability of the prospective parents and their attitudes toward children must also be considered. Such an evaluation of multiple factors is admittedly difficult and inexact, but it must be undertaken.

The genetic counselor has a responsibility to relieve anxiety and fear wherever this is possible, and this may be done adequately only by considering all possible aspects of the unique family situation under observation. This principle and other related matters were discussed at more length during a panel discussion on genetic counseling held at a recent meeting of the American Society of Human Genetics (Dice, 1952). It is to be hoped that further research in genetics and allied fields will permit more exact estimations of the total genetic potential of individuals in the future.

REFERENCE

DICE, L. R., *chairman*, 1952. A panel discussion: genetic counseling. *Amer. J. Human Genetics* 4:332-346.

Annual Business Meeting of American Eugenics Society

The annual business meeting of the American Eugenics Society was held at the Sheraton-Plaza Hotel, Boston, Massachusetts on December 29, 1953, immediately after the Symposium on Genetics Factors Affecting Intelligence, with President C. Nash Herndon presiding. The following new directors were elected: Gordon Allen, M.D., Dr. Emily Mudd, Mrs. Dorothy Iselin Paschal. An amendment to the By-laws was passed which raised membership dues from $3.00 to $5.00 and abolished library and associate membership. The Annual Report (see p. 79) was presented by the Secretary, Frederick Osborne, and the Treasurer's report read.

THE ORIGIN AND EVOLUTION OF MAN[1]

Cold Spring Harbor Symposium 1950

FREDERICK OSBORN

The meetings held in the summer of 1950 at Cold Spring Harbor seem to have marked a turning point in the acceptance by other scientists of work in the field of human genetics. The sponsors of the symposium may have had chiefly in mind that it would be worthwhile to bring together the most recent information on human origins. This was indeed the case.

The papers are published in Volume XV of the *Cold Spring Harbor Symposia on Quantitative Biology*. From the opening paper by Dr. Strandskov on the genetics of human populations, which seems to have taken the chairman of the session rather by surprise, through the three sessions on the genetic analysis of racial traits and the sessions on race concepts and on constitution, the dominant basis of discussion is the human gene. To quote the words of a participating anthropologist, Joseph B. Birdsell: "The problems of human evolution and racial differentiation are essentially population problems, and their solution will be advanced by borrowing techniques of analysis from the vigorous field of population genetics."

For this reason the record of the symposium is immensely stimulating reading for anyone interested in eugenics. Except for the papers of Böök, Kemp, and Neel, the traits chiefly discussed are not of direct eugenic significance. But a discussion of the distribution of the blood groups and of taste discrimination is a lesson in methodology which we may hope will later be applied to the distribution of genes with greater social significance.

The symposium was attended by one hundred twenty-nine leading geneticists and anthropologists from this country and abroad; it lasted eight days and heard twenty-eight papers. This brief review can do little more than indicate the range of material presented and some of the more striking conclusions.

Strandskov's opening paper on "Genetics and the Origin and Evolution of Man" is a model of conciseness and pertinency. Compressed in nine pages, it covers the basis for an understanding of population genetics, with consideration of intra-breeding populations, mating systems, ascertainment of the genetic composition or allelotype of intra-breeding populations, factors making for change, isolation, genetic drift, and the interaction of these various factors in evolution.

The genetic analysis of racial traits was discussed through three sessions and in nine papers. In a way this was the high point of the symposium. At no previous meeting of scientists have so many qualified men brought together in this field so much sound material based on modern statistical techniques. The total makes an impressive picture of the beginning of knowledge about the differences between racial groups and between other smaller population units.

Böök of the Department of Medical Genetics of the University of Lund, Sweden: Kemp of the University Institute of Human Genetics, Copenhagen, Denmark; and Neel of the Institute of Human Biology, University of Michigan contributed papers on the population genetics of inherited defect and tendency to disease. These men are among the leading authorities in the field of medical genetics. Böök gives figures for the rough frequency of various hereditary disorders and defects in the Swedish population as a whole as com-

[1] *Cold Spring Harbor Symposia on Quantitative Biology*, Vol. XV, The Biological Laboratory, Cold Spring Harbor, L. I., N. Y., 1950, 425 pp.

pared to a number of Swedish isolates. Kemp gives tables for the incidence in the population of Denmark, numbering a little over four million inhabitants, of fifteen types of congenital malformations, nine diseases of the internal organs, ten types of hereditary eye diseases, eight kinds of inherited mental illness, and hereditary blindness and deafmutism. Added together these anomalies affect a surprisingly large fraction of the total population.

Neel's paper "raises problems in population genetics that are both complex and baffling at the present time." It deals with thalassemia (Cooley's anemia), thalassemia minor, and sickle cell anemia. The thalassemias are prevalent among a large fraction of people in the Mediterranean area while sickle cell disease manifests itself chiefly among Negroes in the United States. Thalassemia major and thalassemia minor are a severe and mild form of the same disease. There is no doubt of its hereditary nature. Among the siblings of individuals with thalassemia major, the thalassemia major is so severe that those who have it (the homozygous for the gene) always fail to reproduce; the incidence of the gene should therefore be reduced at a constant rate, offset only by the increase caused by new mutations. It should be possible to calculate the incidence of the gene from the proportion of deaths from the disease. But when this calculation is made, the incidence of the gene in Sicily and other Italian areas is so high as to require a disturbingly high estimate of the mutation rate. No satisfactory answer to this problem has been found.

Sickle cell disease is a severe chronic type of anemia prevalent among American Negroes. It is found in persons whose blood shows observable "sickle cell characteristics." But not all persons with this type of blood have the disease. Studies by Neel indicate the existence of a gene which in a single dose elicits the sickle cell phenomenon or blood type, and in a double dose, sickle cell disease. Other studies have confirmed this homozygous-heterozygous hypothesis. Studies made in Africa indicate that in some areas 20 per cent of the population have the sickle cell trait. Since it is a rare event for a woman with the sickle cell trait, in double dose, that is, with sickle cell disease, to give birth to a live child, the gene for sickling should suffer a rapid reduction. Actually the incidence is so high that to maintain it at that level it would be necessary to postulate an impossibly high mutation rate. As in the case of thalassemia, this raises some thorny questions in gene dynamics. There is some evidence from the bones of now extinct human groups that both of these diseases may have played a significant role in the decline of certain racial groups in which they appear to have occurred. In Neel's closing words, "these several situations fairly bristle with loose ends . . . but many of these questions can and should be answered in the next ten years." Their treatment in this symposium is evidence of the quality of the work now being done in the field of population genetics.

The final session on the Genetic Analysis of Racial Traits opened with a paper by Race of the Lister Institute on the eight blood-group systems and their inheritance.

> The blood groups provide the best example of all the basic uncomplicated types of inheritance that Mendel discovered. If the gene is present, then the blood group is present. Blood-group genes are unusually free from the influence of other genes in the body, and free from the influence of sex, of age, of health and of climate. . . . The eight blood groups, together with the ability to taste phenyl thio-carbamide, are the only human physiological characters (apart from sex) of which the manner of inheritance is known with certainty.

Though this opening statement may seem to some geneticists an understatement of their progress to date, Race's paper lays a fascinating basis for further studies, particularly in one of his final statements: "It is quite possible therefore that in the eight blood group systems we have markers for

eight of the twenty-three autosomes of man."

The session on Race Concepts and Human Races discussed the influence of culture upon genetic changes in human populations (Coon); the operation of genetic drift among Australian aborigines and certain ancient gene flows through space in Australia, based on blood groups and other anthropological data (Birdsell); a discussion of the concept of race (Montagu); anthropological maps of the Nordic countries (Lundman); microevolution in Greece, based on 708 skulls, 200 skeletons and 37 living males from before 3000 B.-C. to modern times (Angel). The summary by Dr. Dunn and some of the statements in Montagu's paper are worth quoting because of the light they throw on the views of scientists today on the subject of race.

> Populations can only be defined as races in particular relation to, or in respect of, the gene or genes in the frequency distribution of which they differ from other populations. This is a very different conception of race from that which until comparatively recently prevailed among most zoologists and anthropologists, who were accustomed to thinking of races in terms of absolute phenotypical differences rather than in terms of relative differences in the frequency distribution of traits or genes. (Montagu)
>
> The traditional races of Europe are certainly in very different taxonomic categories from the "races" which at different times peopled the Americas and Oceania. Races change as periods of isolation and interbreeding alternate with periods of expansion and migration and mixture. Consequently race as a noun cannot have a fixed or exact meaning, but only a broad and general one, whereas "racial differences" or "racial mixture" and other adjectival uses convey a real meaning without ambiguity if one thinks of "racial" as referring to the relative and characteristic frequencies, within a group, of a given collection of genes. (Dunn)

The session on Constitution developed some of the most spirited discussion of the entire symposium. It began quietly enough with Garn's discussion of the constiutional modification of Mendelian traits, part of which will be briefly quoted.

> Men whose serum cholesterol exceeds a defined upper limit are more likely to develop atherosclerosis. . . . men whose serum uric acid concentration is in the upper portion of the normal curve are more likely to develop gout . . . high serum concentrations of both cholesterol and uric acid are inheritable, . . . physique alone shows correlations with these constituents . . . Cholesterol showed a significant positive correlation with endomorphy, and a negative correlation with ectomorphy. These correlations were independent of diet. . . . Physique is subject to selection. . . . If a trait is disadvantageous, and if either the degree of expression or the age at appearance varies with physique, then selection will tend to remove those physiques showing either the maximum expression of the trait, or the earliest expression of the trait.

Most of the discussion centered around the papers by Seltzer and Sheldon. Seltzer's 500 male juvenile delinquents were on the average of the mesomorphic or athletic type as compared to the 500 males of the control group, who averaged higher in ectomorphy or the more slender types. His analysis of the corresponding temperamental differences and of their effect on the development of personality is most interesting but was considerably criticized in the discussion which followed. It remained for Sheldon's presentation of his philosophy of the relations between the somatotype, the morphophenotype and the morphogenotype really to arouse the participants at the symposium.

Sheldon assumes that the somatotype and personality are but differently measured and differently presented reflections of the same thing—reflections of bodily structure at different levels of action. "It is my dark presentment that only through general acceptance of (such a point of view) will the great schismatic tragedy of this age be over-

come, and will final human disaster for the time being be averted. The immediate tragedy lies in the schism between biological and social science . . . locked up in the enigma of the somattype there is a clue to the crucial problem of bringing biological and social studies into a single focus."

This point of view and the material presented by Sheldon in its support drew a very lively discussion which was summarized by Hooten thus: "As to the possibility of determining the genetic basis of body type, some geneticists (including Stern) and some anthropologists averred that such a task was possible and exigent, and others asserted that body type is principally phenotypical and as such of no importance to geneticsts." But emphasis on this particular aspect scarcely does justice to Sheldon's highly imaginative, patient investigations in which Dobzhansky sees great possibilities.

In the final session on Perspective of Future Research, Dobzhansky touched on various aspects of human diversity and adaptation. No review could do justice to this able paper. It should be read in its entirety. His account of the polymorphism which enables a population to survive under sudden changes of environments, and his account of the "balanced polymorphism" which is known to occur in *Drosophila* and may be typical of sexually reproducing forms including man—both of these give us a new insight into complexities of the gene distributions which have enabled living things to survive. Dobzhansky's paper ranges over a broad field. A few quotations may indicate its scope and interest:

> Of Sheldon's work he says: — The polymorphism which the students of human constitutions have begun to describe and classify may be produced by a relatively small number, perhaps as few as half a dozen, of genes or integrated combinations of linked genes, with far-reaching pleiotropic effects which extend into every aspect of human morphological as well as physiological and psychic organization.
>
> Medical genetics should not be conceived only as compilation of a catalogue of rare hereditary disorders but also as a study of the reaction norms of different human genotypes in different environments. . . . The most important evolutionary trend in the human species is toward genetically determined educability, *i.e.*, ability to learn from experience and to modify one's behavior accordingly. . . . This trend is the specifically human feature of *Homo sapiens*. . . . Genetically conditioned educability favors diversity, and not uniformity, of human individuals as well as of human cultures. . . . In heterogeneous environments, plasticity results in diversity.

The final paper by Kluckhohn and Griffith on Population Genetics and Social Anthropology is a plea for the unity of the science of man. We think the presumptive evidence strong that the distinctive qualities of each culture and the personality type or types modally associated with that culture will be fully understood only when the genetic predispositions and limitations of that population are described as completely as possible.

We may close the account of this extraordinarily interesting symposium with a paragraph from the concluding remarks of the chairman of the final session, Dr. Curt Stern:

> The emotional weighing of such terms as species, race, delinquency, breed, purity, *eugenics, selection,* and others is heavy. Knowing the terrible harm which has grown out of misconceptions and for which these misconceptions were used as attempted justification, there is a tendency to shy away from conceptual regions, or from terms, which might be exploited by ill-will or stupidity. I believe that science should not yield to such derived impulses. . . . We should not assume that facts do not exist because their uncritical postulation has been detrimental. . . . These thoughts apply particularly to the problems of eugenics. Whether we like it or not, selective forces cannot be excluded from acting in our societies and nations. A thorough study which will enable us gradually to control selection is in the tradition of human cultural evolution.

BOOK REVIEWS

HÖCHSTBEGABUNG (Outstanding Intellectual or Creative Ability): Adele Juda, Urban & Schwarzenberg, München and Berlin, 1953, 182 pp., $2.75.

Lombroso's book, "Genius and Mental Illness," in which mental disorder was thought to be a prerequisite for genius, was the impetus for this exhaustive investigation. Juda studied a representative sample of persons of eminent ability who had made outstanding contributions in the arts and sciences, as well as their ancestors, siblings and descendants. The main purposes of this research were (1) to investigate the relationship between mental disorder and extraordinary ability and (2) to determine the importance of genetic factors in the development of outstanding creative and intellectual ability.

The 294 highly gifted individuals chosen for study lived in German-speaking areas of Central Europe and were born between 1650 and 1900. The 113 artists included sculptors, painters, poets, architects and composers. The 181 scientists consisted of theoretical scientists (historians, educators, etc.), natural scientists, technologists and statesmen. A painter and a poetess were the only women in the group. A control group of 115 artists and scientists of lesser accomplishment, whose personal, marital and genealogical histories were compared with those of the highly gifted probands, was less thoroughly investigated because of wartime conditions. The magnitude of the study is revealed by the fact that some 18,000 persons were investigated. About 5,000 of them were personally interviewed by Juda during a twelve-year period.

The initial part of the book is devoted to a discussion of proband ascertainment. A presentation of statistical data on the probands and their relatives including occupation and fertility follows. The main part of the book deals with both descriptive and tabulated data on the psychopathology of the probands and their relatives, including a number of illustrations from individual but unidentified index cases. Control group data are presented in another section. In a final chapter, the major research findings are summarized with a minimum of theoretical speculation.

The main conclusions were formulated as follows:

1. The process of creating an extraordinarily gifted person is very complex and involves different and not clearly understood combinations of genetic and non-genetic factors. In general, there must be a preparatory ground in the form of certain talents in the ancestry. These talents should combine with healthy character traits. Manual dexterity in the ancestors of artists, rhetorical ability in those of the poets, and philosophic-theologic-didactic talent in the ancestors of scientists are conspicuous predisposing factors in the creation of outstanding ability. Unusual gift in the ancestors of the probands is highlighted by the finding that in only a relatively small percentage of the cases was there but one outstanding person in the family. The number of probands who were related by blood was comparatively high.

2. A combination of several creative faculties such as musical and poetical abilities, musical and painting talents, and sculptural, painting, and architectural abilities occurred frequently in single individuals.

3. A relationship was found between the area of endeavor of the unusually gifted persons and certain mental aberrations. Among the artists, the frequency of schizophrenia (2.8 per cent) was about three times the average expectancy, but manic-depressive psychosis was lacking. Among the artists, 28 per cent were classified as psychoneurotic, while 64 per cent were considered normal. Among the scientists, the frequency of manic-depressive psychosis (4.0 per cent) was about ten times the average expectancy, but there was no case of schizophrenia. Of the scientists, 19 per cent were psychoneurotic and 78 per cent were normal. While the data indicate a certain incompatability between some

mental disorders and specific areas of endeavor, they clearly refute the theory that the genesis of outstanding ability requires the presence of mental abnormality.

4. Highly gifted persons were not found to be at a disadvantage in comparison with the average population in relation to life span, fertility, and tendency to organic illness. Their families showed no trend toward "dying out" while their outstanding ability was reflected in the remarkably high number of intellectually prominent individuals among their children and grandchildren. All the probands were slightly more fertile than their siblings although the scientists and their relatives were comparatively more fertile than the artists and their relatives. Only the relatives of the scientists had a high enough reproductive rate to insure family survival.

5. A comparison of the artists and scientists showed a relative biological inferiority of the former as evidenced by a higher incidence of mental illness in the probands and their families, by a lower fertility rate, shorter life span, increased celibacy and divorce rates of the probands, by a higher number of illegitimate offspring and an increased infant mortality among their children.

6. The incidence of low intelligence was remarkably low in the descendants of both groups of probands.

7. The majority of the wives of gifted persons were excellent marriage companions who contributed directly or indirectly to the outstanding accomplishments of their husbands.

8. The index cases revealed a striking tendency to be first-born and were second-born to a lesser degree. This peculiarity in the order of birth was far more frequent than would be expected if heredity alone was responsible for outstanding ability.

9. The majority of the individuals studied came from thickly populated and racially mixed middle regions of Germany and Switzerland.

This exhaustive study to which Adele Juda devoted 17 years is without major deficiencies. One may have some doubt concerning the accuracy of psychiatric diagnoses in individuals who lived over 250 years ago, irrespective of the fact that no names or detailed biographical data could be offered. In any case, Juda does carefully define her criteria of mental disorder.

With reference to the observed disinclination of extraordinarily gifted people to "die out," Juda's data suggest in light of a generally increasing world population that the proportion of such brilliant persons may be diminishing. Although the control group played a relatively minor role in the study, an interesting finding with reference to this group was that the observed rate of mental disorder was lower than that of the probands. The author might be subject to some criticism for describing the artists as "biologically inferior" to the scientists when this conclusion rests on such variables as increased celibacy and divorce rates, and a greater number of illegitimate offspring. Whatever factors contributed to this finding, they are much too complex to be attributed solely to biological phenomena. In the main, however, it appears that Adele Juda did, indeed, accept the challenge of Lombroso, and through tireless investigation of prodigious proportions was able to refute the popular theory that mental disorder is a prerequisite for extraordinary ability.

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The Limits of the Earth: Fairfield Osborn, Little, Brown & Co., Boston, 1953, 200 pp., $3.50.

In the author's own words: "The purpose of this book is to stress the influence, both as regards past history and the present world situation, exerted by the relationship between people and the resources of the earth. This is indeed the eternal equation — the formula that holds the key to human life, then, now and tomorrow." Mr. Osborn has no doubt that science can continue to make large contributions to human welfare both by showing how resources can be used more effectively and by increasing the control over disease. But

he shows that such a belief does not render useless the discussion of the question whether the future increase of population will not be so fast and so large that the resources available per caput will cease to yield as abundant goods as they now do in the more highly developed industrial areas and may fail to permit of any significant per caput increase in goods in the underdeveloped areas, in spite of the great advances in science that are to be expected.

A survey of the changes which have taken place in the relation of population to resources in several countries which have profited greatly by the application of science to industry leads the author to believe that even in such countries there is doubt that the present level of living can be long maintained if their populations continue to increase. In other countries where the resources are now less fully used the situation is not essentially different because they still have high birth rates with the death rate increasingly coming under control and are likely to have a very rapid population increase for some time after they really begin to use modern methods of production and to spend more on public health.

Mr. Osborn is fully aware that at present the opinions of well-informed men differ greatly regarding what science can do to increase production in the underdeveloped areas and to sustain present levels of production in the more advanced industrial areas. But unlike a good many people who think of science in relation to production in general terms always making the proviso in their own minds, "If present knowledge could be efficiently applied throughout the world," Mr. Osborn is thinking in terms of practical likelihood. Because of this he realizes as many people do not that the *probability* of doubling production in many backward areas is quite a different matter from the *possibility* of doing so, given wholly favorable circumstances. He knows that circumstances will never be wholly favorable.

Because he is so fully aware of the wide gap between probability and possibility in the efficient utilization of world resources, Mr. Osborn is very justly impatient with broad statements to the effect that the world can readily support double its present population or some other large and global figure. He wisely observes: "Out of the welter of conflicting opinions and bold, if not careless, forecasts, only one valid or practical observation can be made. It will be time to conjecture how many people the earth is capable of supporting when the increase in the production from agriculture, or through other means, is demonstrably keeping pace with population growth. This it is not doing now."

This is equivalent to saying that there is no profit in talking in general terms about the support of world population, that the common sense thing to do is to ask how 'A' and 'B' and 'C' with already relatively large populations who have only begun to apply science to their problems of production and health, are going to improve their welfare. Will their numbers increase about as fast as their production? Do they have the resources to which to apply better means of production? etc., etc.?

Mr. Osborn insists that we cannot assume that production will increase faster than population because what could be done under wholly favorable conditions will prove to be quite different from what actually will be done under the existing limitations imposed both by the quantity and quality of resources and the social and economic organization of peoples, which always place fetters on production. The reviewer thinks Mr. Osborn has made a good case for the more careful consideration of the whole problem of the relation of man's numbers to his resources and his future exploitation of them, so good a case that he is fully justified in his conclusion that it is time to consider the deliberate control of population, if any considerable portion of mankind is ever to enjoy a reasonably decent level of living.

Finally, Mr. Osborn has written a highly enlightening and very readable book. The reviewer hopes that it will have a very wide circulation among thoughtful people. It is not a *scare* book. It is sober and reasonable and well-considered in spite of being easy to read.

WARREN THOMPSON

ABSTRACTS

GENETICS

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Differential fertility between women of blood groups O and A, R. L. Kirk *et al. British Jour. Preventive and Social Medicine* **7** (1953), pp. 1-8.

It has been established that women of blood group O married to group A men have fewer children than other women, and this constitutes a strong natural selection against the genes for O and A blood. In order to account for the persisting high frequencies of these two blood groups and of the Rh-negative blood type, some authors have postulated compensatory mechanisms in the reproductive patterns of the respective groups.

The present authors give data on women admitted to a large Australian maternity hospital. Women of group O had a significant excess of pregnancies at the beginning and end, and a deficiency in the middle of the reproductive period. The data support the hypothesis that women of blood group O are innately more fertile than those of blood group A, provided they are not married to group A men. This is a sufficient compensatory mechanism to keep the blood group O at a high frequency but throws no light on compensatory mechanisms for the A gene.

The occurrence of acquired hemolytic anemia in subjects of blood group O, M. L. Hunt and S. P. Lucia. *Science* **118** (1953), pp. 183-184.

The distribution of the ABO blood groups was studied in 58 patients with hemolytic anemia. Among those whose anemia was acquired rather than congenital, there was a significant excess of blood group O in comparison with the other patients and in comparison with the general population. Preliminary experiments suggest that susceptibility to acquired hemolytic anemia may be related to absence of blood group A substance from the saliva.

These two papers illustrate a principle which has just recently begun to impress human geneticists: the mere observation of diversity with respect to a trait like the blood groups is not sufficient evidence that the trait has no importance in survival or is independent of natural selection. If the selective value of even a blood group antigen can be so complicated as these papers would suggest, how much caution is necessary in evaluating more complex hereditary traits?

The dynamics of racial intermixture—an analysis based on the American Negro, Bentley Glass and C. C. Li. *Amer. Jour. Human Genetics* **5** (1953), pp. 1-20.

The authors derive an equation which can be used to analyze the flow of genes from one population into a hybrid population as illustrated by U. S. Whites and American Negroes, respectively. The equation relates the frequency of a particular gene in the hybrid population to its frequency in the two parent populations and permits conclusions either about the rate of admixture or about the number of generations in which mixture has occured. The authors apply the equation to American Negroes to determine the rate of addition of White genes to the Negro gene pool. Gene frequency estimates are taken from the literature excepting those for blood group genes in American Negroes, which are based on a new series of 907 Baltimore

[1] *Papers abstracted in this column are not necessarily offered as the best or the most important papers appearing on human genetics. They are selected because they appear to be relevant to eugenics and comprehensible to non-geneticists; generally they are papers which present definite conclusions or which illustrate important trends in genetic research.*

Negroes. The number of generations of Negro-White mixture is estimated as ten or more. The substitution of these values in the equation for each of several genes yields estimates that are all in fair agreement. The estimate based on one of the Rh genes, R^o (cDe), is considered the most reliable and indicates that admixture has occurred at the average rate of 3.6 per cent per generation. The frequency of this same gene indicates that, already, 30 per cent of American Negro genes are derived from White ancestors. At the calculated rate of gene flow, the Negro and White populations should reach approximate equilibrium in about 1700 years, but important differences between the groups would disappear in perhaps two-thirds of this time. Recognizing that rate of admixture has not been and will not be constant, the authors suggest that the present rate could be determined by studying a large number of American Negroes from the oldest and youngest living generations. They also point out that natural selection for or against the genes involved may sometimes disturb the gene frequencies and alter the process of gene flow.

Elimination of recessive lethals from the population when the heterozygote can be detected, S. M. Gartler. *Amer. Jour. Human Genetics* **5** (1953), pp. 148-153.

The author investigates the long-term consequences of an assortative mating system which would prevent expression of a severe recessive defect by eliminating all matings between persons heterozygous (carriers) for the gene. Since the introduction of this system would effectively stop natural selection against the lethal condition, the gene would at once begin to accumulate in the healthy population by the addition of mutations at the rate characteristic of the gene. This accumulation could proceed only until one-half the population was heterozygous, at which point every homozygous normal (non-carrier) person would have to take a heterozygote as mate. Further mutations superimposed on this 50-50 population would produce extra, unmarriageable heterozygotes and lethal homozygotes. With mutation rates of the order generally accepted for man, several thousand generations would be required for a population to reach this condition with respect to any one gene. But before then the drastic restriction of freedom in choice of mates would make the system intolerable, especially if several conditions were included in the program.

This is a problem that could hardly be viewed with concern in the present century, but the same mating system would raise other problems much sooner. If all prospective marriage partners had to undergo tests even for the few conditions which can now be detected in the heterozygous condition, the cost and confusion would be quite burdensome.

Data on linkage in man: elliptocytosis and blood groups. II. Family 3. H. B. Goodall *et al. Annals of Eugenics* **17** (1953), pp. 272-278.

The authors present data on a family showing hereditary transmission of an uncommon condition in which the red blood cells are oval instead of round. Although they studied only six affected and twelve unaffected individuals, they found a perfect association between the trait and one of the genes determining Rh blood type. This suggests very close genetic linkage between the respective genes. However, the authors point to the companion article which described two families with apparently the same hereditary condition, and in these families there was no evidence at all for such linkage.

The authors point out that, for the purpose of detecting carriers of hereditary disease, linkage is not of value unless it is almost complete, as in this family. Even when such complete linkage has been established for one family, it cannot be safely assumed for other unrelated cases.

Homologous sex-linked mutations in man and other mammals, F. B. Hutt. *Amer. Naturalist* **87** (1953), pp. 160-162.

The author called attention in 1948 to the remarkable circumstance that the mutation causing hemophilia is sex-linked in dogs, as in man. Recently a mutation has

been described in calves which is recognized by deficient development of hair, skin-glands and teeth. An almost identical condition is known in man and, again, the hereditary factor is sex-linked in both species. The author refrains from speculation but points out that the occurrence of two such parallel cases can hardly be a mere coincidence. Perhaps the other sex-linked recessive traits in man have sex-linked counterparts in other species.

Without indulging in the speculations from which Dr. Hutt refrained, we can at least remember this as a useful illustration of the close parallelism between human genetics and the genetics of experimental animals.

A human blood-group chimera. I. Dunsford *et al. British Medical Jour.* **2** (1953), p. 81.

In the course of routine typing of blood donations, one specimen was found to contain a mixture of two kinds of red blood cells. The cells differed with respect to three known factors, chief of which was the A factor in the ABO system. Sixty-one per cent of the cells were of blood group O, and 39 per cent were of blood group A. The blood donor was a 25-year-old woman; when questioned, she revealed that she had had a twin brother who died in infancy.

There has been no previous report of a human showing a double set of blood groups, and this instance can undoubtedly be ascribed to the prenatal condition as a twin. But even in twins the phenomenon must be unusual. This woman was not a freemartin because she had a living child, but if other instances occur, they may show some reduction in fertility.

By analogy with cattle twins, where this condition occurs regularly, it can be assumed that this woman acquired one set of blood groups from her twin through vascular anastomoses in the placenta. The primordial blood cells of the twin would have become established, along with her own, in her blood-forming tissues. In cattle, when a female twin has a male partner, her sexual development is incomplete and she is known as a freemartin.

The calculation of reproductive fitness and the mutation rate of the gene for chondrodystrophy, R. E. Popham. *Amer. Jour. Human Genetics* **5** (1953), pp. 78-75.

In 1941 an erroneous method of calculating reproductive fitness was used in estimating the mutation rate of the gene for chondrodystrophic dwarfism. The error has been overlooked and even repeated. The fecundity of the dwarfs was measured in terms of their abnormal children only, or one-half of their offspring. This figure was compared with the total fecundity of their normal siblings, and indicated a relative reproductive fitness of only 0.098 for the dwarfs. When all children are taken into account, the correct reproductive fitness is found to be 0.196. This figure, together with other data, yields an estimate for the mutation rate, 8.54 per 100,000 popuation, which agrees closely with an estimate obtained by another method.

POPULATION

CLYDE V. KISER
Milbank Memorial Fund

Parental participation in mate selection and its effect upon family continuity, Marvin B. Sussman. *Social Forces,* **32,** No. 1 (Oct. 1953), pp. 76–81.

"The purpose of this paper is to illustrate from empirical data how parents attempt to affect marriage partner choices of adult children and the consequences of their action upon the continuity of relationships between parents and children's families after marriage." (p. 76)

This paper is based upon materials taken from the author's Ph.D. dissertation, Family Continuity: A Study of Factors Which Affect Relationships Between Families at Generational Levels (unpublished, Yale University, 1951).

As for the data "During 1950, parents of 97 families of the metropolitan area of New Haven, Connecticut who had 195 married children living away from home were intensively interviewed regarding relationships with their children. On their social characteristics they were typically

urban middle class, Protestant, white, economically prosperous, and well educated. The sample was randomly selected from a list of 438 families who fitted the above description. Seventeen ministers and a number of respondents suggested these names." (p. 77)

Among the findings the author reports: "Parents in this study actively influenced the marriage choices of their children. They did this by provision of parties, weekend outings, vacations, and other activities for the children and their friends. Parents who were unable to furnish such entertainment for them used other means to control their courtship, relying upon cajolery, persuasion, appeals to loyalty, and threats to deter them from what they judged to be 'poor' marriages. Our data suggest that when parents have positive roles in the mate choices and courtships of their children the objective of family continuity is most likely to be achieved." (pp. 80-81)

Age at marriage and proportions marrying, John Hajnal. *Population Studies,* **7,** No. 2 (Nov. 1953), pp. 111–136.

In this article the author is concerned with the wartime and postwar increase in marriage rates that have been observed in many western countries. The chief data used are those concerning recent trends in percentages single, by sex and age.

"The purpose of the paper is to use these data to disentangle two effects in the 'marriage boom': (1) the extent to which the cohorts whose marriages have formed the main contribution to the 'marriage boom' are marrying more, *i.e.,* whether the proportion who will ultimately remain single is likely to be lower than in previous generations; (2) the extent to which the mean age at marriage of these generations is likely to fall below that of their predecessors."

As for results, the data indicate that changes of both types have taken place. "While a decline in the singulate [mean] age at marriage at the rate of 0.1 year per annum is taking place, the number of marriages every year is roughly 10 per cent greater than before or after the decline." (p. 123)

According to Hajnal's figures, the marriage boom has been especially pronounced in the United States. Thus among males 20–24 years of age the proportion single declined from 72 per cent in 1940 to 52 per cent in 1951. Among females 25–29, the proportion single declined from 23 per cent in 1940 to 11 per cent in 1951.

Hajnal's paper is of value from the standpoint of methodology as well as content.

Is a new family form emerging in the urban fringe? E. Gartly Jaco and Ivan Belknap. *American Sociological Review,* **18,** No. 5 (Oct. 1953), pp. 551–557.

The authors begin with the statement "One modern school of family sociologists, represented chiefly by Zimmerman, considers the present urban American family an alarming instance of disintegration in the familial processs. This disintegration is believed to have reached such extremes that the family can no longer adequately discharge vital functions such as reproduction and socialization." (p. 551)

The aim of the paper by Jaco and Belknap is "to point out certain trends that may be leading toward the emergence of a variant type of urban family which may be able to maintain sufficient fertility and integration to satisfy the Zimmerman requisites and yet function adequately in the urban community. This variant type of urban family seems to be locating in the urban 'fringe.'" (p. 551)

The following is one of the nine hypotheses suggested by the authors for further research:

"When contrasted to the central city, the fringe presents the following differential demography: higher fertility, larger families, more marriages, more children, greater number in the labor force, more home ownership, greater fluidity, less mobility, lower mortality, more aged persons, lower age at marriage." (p. 557)

The Structure of mortality in consanguineous families (Structure de la Mortalité dans les Familles Consanguines). Jean Sutter and Léon Tabah. *Population* **8,** No. 3 (July-Sept. 1953), pp. 511–526.

To study this problem, mortality data for two French departments, Morbihan and

Loir-et-Cher, were analyzed. Both of these departments are mainly rural, but they differ with respect to birth and death rates, migration, the frequency of consanguineous marriages, percentage of foreigners, etc.

The following conclusions were reported by the authors:

1. Mortality is higher among descendants of consanguineous families than among descendants of the nonconsanguineous families.

2. The excess mortality in the consanguineous families is considerable among infants under 10 days old (*i.e.*, perinatal mortality is considerably higher among the consanguineous families than among the nonconsanguineous). At later ages the excess mortality of the consanguineous families is less marked although it remains appreciable and the distribution of the deaths by age is the same as in the nonconsanguineous families.

3. In all families mortality increases with size of family, but this type of relation is stronger in the consanguineous than in the nonconsanguineous families.

4. Certain families are more affected than others by mortality. This phenomenon is observed to some extent in the nonconsanguineous families; it is strongly multiplied in the consanguineous families.

In general, the authors interpret their results to mean that genetic factors have played an important role in the general lowering of mortality rates and in the increase in life expectation during the past two centuries.

Some estimates of the general age distribution, fertility and rate of natural increase of the African population of British East Africa, C. J. Martin. *Popuation Studies,* 7, No. 2 (Nov. 1953), pp. 181-199.

In view of the paucity of demographic data for Africa it is helpful to have even rough estimates. The 1948 Census of British East Africa (Kenya, Tanganyika, and Uganda) enumerated 17.8 million people of which 98 per cent were Africans. If, as was usually the case, the Africans did not know their precise ages, "an attempt was made to obtain ages by reference to historical events. . . . Enumerators were trained to estimate ages by reference to historical events when ages were not known and to place individuals into five main categories: Under 1 year, 1–5 years, 6–15 years, 16–45 years, and 46 years and over. In addition, a special column was provided for the actual age, if known." (p. 184)

Utilizing these broad categories the percentage age distributions were computed separately for Kenya, Tanganyika, and Uganda. The distributions were found to be fairly similar for these areas and also fairly similar to those for several other countries at indicated times: Guatemala (1940), Peru (1940), Turkey (1945), Brazil (1940), and Palestine Moslems (1931). On the basis of these materials a "very rough free-hand curve of a possible age distribution of [all] East Africa has been drawn, and it is believed that the results are not inaccurate and can be taken as rough guides to the age distribution of the population in East Africa." (p. 188)

Regarding the vital rates, the author says "A death rate of 25 per 1,000 would seem to be the minimum, and the rate may well be over 30 per 1,000 in certain areas. . . . The fertility of African women, while not low, is not as high as found among certain other communities. It is, however, high enough, should the death rates fall, to result in a tremendous increase in population. There would appear to be no reason at present why fertility rates should fall, and it is even possible that with improved conditions they may rise." (p. 199)

THE AMERICAN EUGENICS SOCIETY
Annual Report 1953

GROWING WORLD CONCERN with population problems gives special emphasis to the significance of the work of the American Eugenics Society. The position of eugenics today is fortified by such markers as the increasing interest in medical genetics, the formation of a eugenics section in the 1954 World Population Conference, the Pope's recent endorsement of positive eugenics, the need to balance the concern over size of population with concern for the quality of that same population. Misinterpretations of the nature of eugenics which have clouded its good name in the past are gradually being replaced by more clear-sighted views. To encourage eugenic trends at this point in the life of our nation seems as vital a task as any awaiting our hand.

Research

Medical Handbook: The Society has raised funds for the writing of a medical handbook on genetic defect for the practicing physician, under the sponsorship of the New York Academy of Medicine. Dr. C. Nash Herndon, President of the Society, has been preparing material during 1953 for the handbook, and it should be ready for publication in a year or two.

Twin Study Proposals: The Society is studying a proposal for a major project on the study of twins which it plans to develop during the next few years. A small grant has been made to the Society to finance an initial planning conference, and an effort will be made later to interest some of the larger foundations in the project.

Publications

Journal: The Society has expanded its journal, EUGENICAL NEWS, from 16 pages to 64 pages under the leadership of the Editorial Board. As of March 1954, the name of the journal will be changed to EUGENICS QUARTERLY, and the contents will be directed to both scientists and laymen.

Reprints and Pamphlets: The Society has distributed over 8,500 pamphlets and reprints during 1953. This is a new phase of the publications program which has far outstripped the circulation of the journal itself and promises to be an important part of our educational effort. One of the most surprising developments of the past year has been the demand for the Society's printed materials. For the first time these are being listed regularly in publications which have wide circulation. Requests for copies of individual publications were received at the following rate during the four quarters of 1954, making a total of 1951 individual requests.

1st Quarter	2nd Quarter	3rd Quarter	4th Quarter
18	130	342	1461

Membership

During this year the Society has commenced strengthening its membership. It is hoped that the membership of the Society will double or triple during the next year.

Meetings

For the first time in a number of years the Society conducted a symposium (with the American Society of Human Genetics) at its annual meeting in Boston, Mass., December 29, 1953, in conjunction with the AAAS meeting. Similar programs will be developed for future meetings.

Plans are also being formed to organize eugenic sections in some of the larger meetings of scientific and family-life groups.

The Society sees 1954 as a year of expanded activity in every branch of its program. It expresses its gratitude to Dr. M. A. Bigelow for his past leadership.

Respectfully submitted,
FREDERICK OSBORN
Secretary

December, 1953

American Eugenics Society

230 Park Avenue
New York 17, N. Y.

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Trends in Capacity for Intelligence, Frank Lorimer
New Directions in the Genetic Study of Intelligence and Personality, J. P. Scott
Fertility Problems in the World Today, Abraham Stone, M.D.
Contribution of Planned Parenthood to the Future of America, Frederick Osborn
Counseling Centers on Human Heredity, Lee R. Dice
Address of His Holiness Pope Pius XII to those attending the *Primum Symposium Geneticae Medicae* (Sept. 7, 1953).
Major Policies of the American Eugenics Society
Possible Effects of Differential Fertility on Genetic Endowment, Frederick Osborn
Eugenic Hypothesis, Frederick Osborn

Eugenics and Health

Eugenics and Preventive Medicine, C. Nash Herndon, M.D.
Lethal Genes: A Factor in Fertility, Robert C. Cook
Rh Blood Factor: Genetic Aspects of Erythroblastosis, Leon Sussman, M.D.
Heredity in the Prevention of Chronic Disease, James V. Neel, M.D.
Inheritance of Cancer in Man, Madge Macklin, M.D.

Eugenics and the Family

Preparing for Parenthood, Frederick Osborn
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The American Eugenics Society

Program

To promote research on the interaction of biological and social factors in human development and on conditions influencing the distribution of births.

To integrate scientific findings in human genetics and the social sciences in order to determine the influences which can be used to develop a constantly improving selection by births to replace the harsh and wasteful methods of natural selection by deaths.

To cooperate with the medical profession in reducing the burden of constitutional weaknesses and defects.

To encourage conditions and attitudes tending to reduce the number of unwanted children and especially of those conceived with indifference to the responsibilities of parenthood.

To encourage conditions and attitudes tending to increase the proportion of children born with promise of sound character and good intelligence.